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ROYAL ASTRONOMICAL SOCIETY.

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PROFESSOR CAYLEY, F.R.S., President, in the Chair.

J. Bridson, Esq., Bolton;
T. W. Bush, Esq., Cromwell Street, Nottingham;
W. H. Finlay, Esq., Royal Observatory, C.G.H.; and
Lieut. G. F. Guyon, St. Leonards-on-Sea,

were balloted for and duly elected Fellows of the Society.

The following Note is inserted by special order of the Council:—

The attention of the Council has been directed to certain remarks made by their late Editor, in paragraph 2, page 583, of the Supplementary Number, Vol. xxxiii. of the *Monthly Notices*. The Council have entered on their minutes a resolution expressing their strong disapprobation of the paragraph referred to.

On the rejection, in the Lunar Theory, of the term of Longitude depending for argument on eight times the mean longitude of Venus minus thirteen times the mean longitude of the Earth, introduced by Professor Hansen; and on the effect of that rejection upon the state of the Lunar Tables, and upon the lunar calculations which serve as basis for Ancient Chronology.

By George Biddell Airy, K.C.B., Astronomer Royal.

In a note, dated 1827, December 16, to a paper published in the *Philosophical Transactions* for 1828, I announced that I had discovered equations of long period in the movements of the

Earth and *Venus* depending on the argument eight times the mean longitude of *Venus minus* thirteen times the mean longitude of the Earth, of which the period is 239 years; and I gave approximate values of the co-efficients. In a paper, dated 1831, November 8, published in the *Philosophical Transactions* for 1832, I gave the details of the computations. The whole process has been reverified, first by M. le Comte G. de Pontécoulant, secondly by M. Le Verrier, and is found to be correct.

From the first announcement of the discovery of this planetary equation, mathematicians, who were more specially engaged on the lunar theory, considered it probable that there would be found an inequality in the geocentric motion of the Moon depending on the same argument. In the summer of 1829, I visited M. Carlini, in the neighbourhood of Milan, and he immediately made inquiries of me as to the possible existence of such an inequality in the lunar motion. In a paper by M. Poisson, "sur le mouvement de la lune autour de la terre," dated 1833, June 17, and published in the *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, tome xiii. 1835, there was given a calculation of the lunar inequality, which led M. Poisson to the conclusion that the inequality in the Moon's motion is insensible. It was, however, subsequently pointed out by M. Delaunay that Poisson's calculation was based only on the disturbance of the excentricity of the Earth's orbit, whereas the effect of the disturbance of the major axis of the Earth's orbit was more important. In this state the question rested for many years.

In 1847, Professor Hansen announced to the French Academy of Sciences (*Comptes Rendus*, tome xxiv. page 795), that he had discovered two equations of long period in the Moon's motion, depending respectively on the arguments 18 × mean long. *Venus* − 16 × mean long. Earth − Moon's mean anomaly, and 8 × mean long. *Venus* − 13 × mean long. Earth, with periods respectively of 273 and 239 years. At the same time, Professor Hansen communicated to the Royal Astronomical Society a statement of the same general character, of which an extract is published in the *Monthly Notices* of the Society for 1847, May 14. Subsequent investigations (of which, I believe, no detailed accounts are published) induced him to make some alterations in the co-efficients first adopted; and, finally, in the Lunar Tables, Professor Hansen adopted the following terms:—

$$+ 15''\cdot34 \times \sin\{18.\ \text{mean long. } \textit{Venus} - 16.\ \text{mean long. Earth} - \text{Moon's mean anomaly} + 30^\circ\ 12'\}$$
$$+ 21''\cdot47 \times \sin\{8.\ \text{mean long. } \textit{Venus} - 13.\ \text{mean long. Earth} + 274^\circ\ 14'\}.$$

The tables, affected with these equations, have been used by myself in the computations of the Eclipses of Thales, Larissa, Agathocles, and Sticklastad, *Memoirs of the Royal Astronomical Society*, vol. xxvi. page 131, &c.

The first of these equations has been recalculated by M. De-

launay. After a first failure (*Comptes Rendus*, tome xlix. page 293), the examination was repeated, and every step of the investigation is printed with the utmost detail, in the *Additions* to the *Connaissance des Temps*, 1862. M. Delaunay's result agrees substantially with Professor Hansen's. It is unnecessary, therefore, to allude further to this inequality.*

The second equation, also, depending on 8 × mean long. *Venus* − 13 × mean long. Earth, was recalculated by M. Delaunay, but with very different result. His investigations, which are given in detail in the *Additions* to the *Connaissance des Temps* for 1863, lead to the conclusion that the direct effect of *Venus* upon the Moon produces an equation of the form above-mentioned with coefficient 0″·0039; and that the indirect effect of *Venus*, by altering the Earth's distance from the Sun and its heliocentric longitude (especially the distance), and thus modifying the action of the Sun upon the Moon, produces an equation with coefficient 0″·2723. I am informed, in private communication from Professor Newcomb, that he has repeated the investigations with nearly the same result. These coefficients, in comparison with that found by Professor Hansen, may be described as insignificant.

Assuming then that the equation

$$+21''.47 \times \sin\{8.\ \text{mean long. } \textit{Venus} - 13.\ \text{mean long. Earth} + 274^\circ\ 14'\}$$

ought to be withdrawn from Hansen's tables, it becomes matter of interest to ascertain what will be the effect of that withdrawal upon the Lunar Tables and upon the theory embodied in those tables. For this purpose, I must refer to my paper in vol. xxix. of the Society's *Memoirs*, "Corrections of the Elements of the Moon's Orbit, &c.," and specially to the section, beginning on page 9, and headed, "Values of R. and Correction of Moon's Epoch of Longitude and Mean Motion." It must be remembered that the Moon's observed longitudes have been compared with longitudes computed, principally by use of Damoiseau's Tables, with Damoiseau's Mean Longitudes, on Plana's Theory; that the results of the comparisons have been collected in groups of years; and that for each group the corrections of certain elements are computed. The only element which concerns us here is the Moon's Mean Longitude. The correction which must be applied

* I cannot dismiss this remarkable equation without expressing my opinion that its discovery is one of the greatest steps—perhaps the greatest step—in Gravitational Astronomy. My experience in the calculation of the planetary equation depending on {8. mean long. *Venus* − 13. mean long. Earth} enables me to appreciate the labour of Professor Hansen's calculation. The research by which it was suggested must also have been laborious. It may be interesting to the student of these investigations to remark that the final value of the coefficient depends almost entirely on the inclination of the orbit of *Venus* to the ecliptic. The relative position of the perihelia of *Venus* and the Earth, and the smallness of the excentricities, greatly diminish the terms independent of inclination. I had pointed out this circumstance in the planetary equation to which I have alluded.

to Damoiseau's Tabular Mean Longitude, in order to produce the Mean Longitude deduced from observation, is called R.

Now this comparison with Plana's Theory and Damoiseau's Mean Longitudes was made before the suggestion by Hansen of the two equations of long period produced by the action of *Venus*. Regarding the results of the means of theoretical terms of short period through a few years, as being essentially the same in the two theories, the mean longitude being always subject to correction, we could then add the numerical values of Hansen's two equations to Plana's Theoretical Longitudes, or we could subtract the values of the two equations from the values of R, in order to ascertain the true correction to Damoiseau's Mean Longitudes, which were used in the comparison of observation with theory. This process was used in the paper to which I have referred, and a series of values of "R – Hansen's Inequalities" is formed on page 9 of that paper, which it is not necessary to reprint here. It is sufficient to state that it was found on page 10, that the errors of the groups might be expressed in the mass by the formula $-2''{\cdot}48 + 2''{\cdot}771 \times$ No. of group, measured from the middle group, and that, on substituting in this formula, the residual errors for the individual groups were not unreasonably large.

But the rejection of one of Hansen's equations invalidates the latter part of this process. It is necessary now to use only the inequality depending on $18 \times$ mean long. *Venus* $- 16 \times$ mean long. Earth – Moon's mean anomaly. Mr. Christie has kindly computed for me the value of this equation for the middle of each group; and the determination of the correction to the Tabular Mean Longitude now stands as follows:—

Range of years in each group.	Value of R given by the equations.	Hansen's inequality of long period.	R—Hansen's inequality.
	″	″	″
1750 to 1759	− 3·19	+ 6·55	− 9·74
1755 to 1764	− 1·38	+ 4·91	− 6·29
1760 to 1768	− 0·31	+ 3·38	− 3·69
1765 to 1773	+ 1·43	+ 1·64	− 0·21
1769 to 1778	+ 3·04	+ 0·06	+ 2·98
1774 to 1782	+ 3·40	− 1·53	+ 4·93
1779 to 1787	+ 3·04	− 3·27	+ 6·31
1783 to 1791	+ 3·76	− 4·63	+ 8·39
1788 to 1796	+ 4·48	− 6·28	+ 10·76
1792 to 1801	+ 2·76	− 7·69	+ 10·45
1797 to 1805	+ 0·87	− 9·02	+ 9·89
1802 to 1810	+ 1·13	− 10·38	+ 11·51
1806 to 1815	+ 0·93	− 11·49	+ 12·42
1811 to 1819	− 0·61	− 12·49	+ 11·88
1816 to 1824	− 1·39	− 13·42	+ 12·03
1820 to 1829	− 1·11	− 14·13	+ 13·02
1825 to 1833	− 1·15	− 14·66	+ 13·51
1830 to 1838	− 1·58	− 15·08	+ 13·50
1834 to 1842	− 1·01	− 15·26	+ 14·25
1839 to 1847	+ 0·15	− 15·31	+ 15·46
1843 to 1851	+ 1·87	− 15·21	+ 17·08

The values in the last column may be best represented in a simple formula (as before), expressing the correction applicable to mean longitude and mean motion, by the expression $+ 8''{\cdot}02 + 1''{\cdot}097 \times$ No. of group, measured from the middle group. The following table exhibits the result of substitution in this formula :—

No. of Group.	Formula	Excess of R—Hansen's Inequality.	No. of Group.	Formula	Excess of R—Hansen's Inequality.	No. of Group.	Formula.	Excess of R—Hansen's Inequality.
	"	"		"	"		"	"
−10	− 2·95	− 6·79	− 3	+ 4·73	+ 3·66	+ 4	+ 12·41	− 0·38
− 9	− 1·85	− 4·44	− 2	+ 5·83	+ 4·93	+ 5	+ 13·51	− 0·49
− 8	− 0·76	− 2·94	− 1	+ 6·92	+ 3·53	+ 6	+ 14·60	− 1·09
− 7	+ 0·34	− 0·55	0	+ 8·02	+ 1·87	+ 7	+ 15·70	− 2·20
− 6	+ 1·44	+ 1·54	+ 1	+ 9·12	+ 2·39	+ 8	+ 16·80	− 2·55
− 5	+ 2·54	+ 2·39	+ 3	+ 10·21	+ 2·21	+ 10	+ 17·89	− 2·43
− 4	+ 3·63	+ 2·68	+ 2	+ 11·31	+ 0·57	+ 11	+ 18·99	− 1·91

It appears, therefore, that after applying the best correction that we are able to give to the assumed mean longitude and mean motion, there still remains a series of unexplained discordances, varying from $- 6''{\cdot}79$ to $+ 4''{\cdot}93$. And each of the discordances here exhibited is based upon about 440 observations; these observations are all reduced by the same sytem of clock-stars and other elements (Bessel's in the *Tabulæ Regiomontanæ*) ; and the tabular places are all computed by the same theory (Plana's, with Damoiseau's epochs).

I conceive it to be totally impossible that a complete and correct theory can leave such large and systematic discordances. And I express my opinion that there is still some serious defect in the Lunar Theory.

Supposing the formula above given to be accepted as a true exhibition of the correction required to Damoiseau's epochs of longitude, it would appear that (remarking that the mean interval of groups is 4·65 years) Damoiseau's annual motion ought to be increased by $0''{\cdot}236$, or his secular motion ought to be increased by $23''{\cdot}6$. In the former investigation (*Memoirs*, vol. xxix., page 10), Damoiseau's secular motion was increased by $59''{\cdot}6$; and, as I understand, Hansen's secular motion was adopted from Damoiseau's, thus corrected. It would appear, therefore, that (still on the supposition that the formula above-mentioned is accepted) the secular motion of Hansen's Tables ought to be diminished by $36''$. We have now to examine the effects of this diminution on the eclipses of antiquity, which have been computed with Hansen's Tables.

The term which has been removed from the Tables vanishes about the year 1827. The eclipse of Agathocles occurred in the year −309; that of Larissa (as I accept it) in −556; that of Thales in −584. The intervals backwards, through which the altered

motion in longitude applies, are, therefore, 2136, 2383, and 2411 years respectively. The alterations of Moon's motion in longitude (adopting the formula of the last paragraphs) are 12′ 49″, 14′ 18″, 14′ 28″. The tabular places of the Moon will be increased by these quantities.

In my paper 'On the Eclipse of Agathocles, the Eclipse at Larissa, and the Eclipse of Thales,' published in the Society's *Memoirs*, vol. xxvi., page 147, I have remarked that the diminution of Secular Acceleration, from the value employed by Professor Hansen to that proposed by Professor Adams, and supported by the investigations of Delaunay and others, would diminish the tabular longitude of the Moon by 160″ for 1000 years. The effect for the intervals of years above given (proportional to the square of the time) would be 12′ 10″, 15′ 8″, and 15′ 30″. This effect, therefore, so nearly balances the other, that the general result may be thus stated. The eclipses, as described in the paper to which I have referred, are accounted for almost equally well, either by Hansen's Tables, including the equation now rejected, and including also Hansen's secular acceleration; or by Hansen's Tables, deprived of that equation, and combined with a secular acceleration diminished to the extent which I have indicated.

My confidence, however, in the certainty of chronological results, derived from lunar calculations, is in some measure shaken by the character of the unexplained inequality in the epochs of Moon's mean longitude, to which I have called attention.

Royal Observatory, Greenwich,
1873, *October* 8.

On the Correction to Hansen's Semi-Diameter of the Moon from Occultations of Stars. By E. Neison, Esq.

In the Appendix to the volume of *Greenwich Observations* for 1864, Sir George Airy has shown, from the computations of Mr. Breen, that the value for the Moon's semi-diameter given by occultations of stars by the Moon, is less than the apparent telescopic semi-diameter by more than two seconds of arc. As the value for the mean telescopic semi-diameter was derived from the Greenwich observations, and was regarded as an accurate determination of the Moon's semi-diameter as viewed with the Greenwich instruments, this difference was looked upon with considerable interest.

Hansen's value for the semi-diameter of the Moon being regarded as expressing very accurately the true semi-diameter of the Moon, it became a matter of interest to determine whether any similar difference occurred with regard to this value and that given by occultations of stars by the Moon. It was, moreover, desirable to extend the period embraced by Mr.

Breen's computation from the end of 1860 up to 1870, the latest period available; thus increasing the number of phenomena in each class, which was rather small for the purpose of an investigation of this kind.

The method adopted was the same as that described by the Astronomer Royal in the Introduction to Mr. Breen's Results (*Greenwich Observations*, 1864, Appendix 1), and therefore does not require to be detailed here.

The corrections to the tabular R.A. and N.P.D. of the stars were obtained from the following sources. For the years 1861 to 1868, from the Annual Catalogue in the *Greenwich Observations* for each year, and from the Greenwich Seven Year Catalogues for the epochs of 1860 and 1864; the mean being taken by giving a proportional weight in each case according to the number of observations. For the years 1868 to 1870 the places were taken from the *Greenwich Observations* for those years, and from the New Seven Year Catalogue, the mean value likewise being taken, with due allowance for the number of observations. The places of two stars had to be derived solely from the Greenwich Twelve Year Catalogue, and those of two others in the first division depend on observations made during 1868 and 1869, contained in the *Greenwich Observations* for those years.

The corrections to the tabular places of the Moon are derived in every case from the observations made during the same day at Greenwich, and are obtained by changing the sign of the tabular errors of the Moon's place given in the *Greenwich Observations*. With ten exceptions the mean of the errors in the Moon's place, according to the transit-circle and altazimuth was taken; and in these exceptional cases the value given by the altazimuth has been adopted, as no observations with the transit-circle had been made.

On substituting these values of e, f, x, and y in the well-known final equation given in the *Greenwich Observations* :—

$$\text{Tabular semi-diameter} - \text{computed semi-diameter} = A \times e + B \times f + C \times x + D \times y;$$

A B C D being the coefficients multiplying the corrections to the places of the Moon and star, and adding the sum of the products to the quantity on the left side of the equation with its sign changed, we have at once the correction to the tabular semi-diameter.

As during the years 1862 to 1870 the values used in the Greenwich reductions of occultations are Hansen's, this gives, without anything further, the correction to Hansen's semi-diameter. For the year 1861, however, the values for the Moon's semi-diameter and parallax differ from Hansen's, and it becomes necessary to apply a further correction. The Greenwich semi-diameter used exceeds Hansen's by $1''\cdot21$, which must therefore be subtracted from the correction given by the above equation.

Hansen's value for the parallax is less than the value employed

for the tabular places of the Moon in the Greenwich reductions of occultations for 1861, by 2″·7 : giving a value to the function denoted *m* in the Greenwich notation of −0·7894 ; which, multiplied by its proper coefficient, must be applied to the correction obtained as described above.

The resulting corrections to Hansen's semi-diameter were finally classified under four heads ; but it was found necessary to omit three or four in which the correction was greater than eight seconds, the reason for which, with one exception, being found in the remarks attached to the observations.

The mean of the results is as follows :—

I. Disappearance of stars at the Dark Limb.

Mean correction to Hansen's semi-diameter from 35 observations −1″·70.

II. Disappearance of stars at the Bright Limb.

Mean correction to Hansen's semi-diameter from 11 observations +1″·81.

III. Reappearance of stars at the Dark Limb.

Mean correction to Hansen's semi-diameter from 20 observations −0″·36.

IV. Reappearance of stars at the Bright Limb.

Mean correction to Hansen's semi-diameter from 10 observations +1″·31.

The observations of the phenomena in Classes II. and IV. are of so very delicate a nature, and are comparatively few in number, that less reliance can be placed on them than on the other two : or to use the words of Sir George Airy in his Introduction to the Results of Mr. Hugh Breen's Computations :—"Of these means, the first and third are (from the nature of the observation) very far superior to the second and fourth, and the first is greatly preferable to the third. The second and fourth classes, as might be expected, give an occultation semi-diameter larger than the first and third class."

As a result, we have a correction to the value of Hansen's for the Moon's semi-diameter exceeding one-and-a-half seconds of arc, derived from the most reliable class of occultations, namely, disappearances at the Dark Limb ; and there would appear difficulties in explaining this by the effects of irradiation. For in the discussion of the eclipse of the Sun of December 21, 1870, the observations with the great equatorial indicate that the effect of irradiation, when exerting its maximum effect in reducing the semi-diameter, only diminishes Hansen's value for the semi-diameter by half a second of arc. The true value for the Moon's semi-diameter it would appear, therefore, cannot be less than this would indicate, while the occultation semi-diameter is considerably less.

The question as to the probable cause of this difference is under investigation; and for the purpose of better treatment, a number of occultations of stars, observed at various observatories, are undergoing reduction, so as to obtain a greater number of phenomena to elucidate the question.

On the Rejection of Discordant Observations. By E. J. Stone, Esq., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Monthly Notices* for April 1873, there appears a paper upon this subject by Mr. J. W. L. Glaisher. Mr. Glaisher states that the original object of his paper was to examine a criterion for the rejection of discordant observations, which I had published in the *Monthly Notices* for April 1868. I have read Mr. Glaisher's paper with care, and venture to offer the following remarks on the subject.

The objections to my criterion appear chiefly to resolve themselves into a preference for a general method of treating observations, which Mr. Glaisher has developed at considerable length. This method is regarded as a complete method of treatment of observations by the theory of errors, and is considered to render the rejection of anomalous results unnecessary. But as Mr. Glaisher supposes that before his treatment is applied 'obvious mistakes' are rejected, and my criterion refers only to the rejection of mistakes, I can hardly understand in what manner the proposed treatment is to render unnecessary my, or some other, criterion for the rejection of mistakes. There are no distinct marks upon the mistakes which appear amongst the records of observations, by which they can be infallibly recognized and rejected.

With respect to the statement made by Mr. Glaisher 'that the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth,' I must confess that this statement appears to me obviously incorrect. The deviations from the truth which may be found amongst observations will depend upon the accuracy of the observations. Deviations will frequently appear in one class of observations made with one class of instruments, which would in the same class of observations made with superior instruments be, I presume, amongst those 'obvious mistakes' which Mr. Glaisher would at once reject. When mistakes appear amongst the records of observations, they can only, I believe, be recognized by a consideration of their deviation from the supposed truth *in comparison with the average discordances of the class of observations under consideration*, and then they can only be recognized with more or less probability and not with absolute certainty.

I believe that most observers and experimentalists will admit

that the number of mistakes which will be found amongst the records of observations will depend upon the carelessness of the observer, the complexity of the observations, the clearness with which the scales, or micrometer-heads, to be read are divided, and the circumstances of illumination of these scales under which the observations have to be made. I believe also that it will be admitted by such persons, that a discordant result, which appeared amongst the records of observations, was more likely to have arisen from a mistake, if the observations were made under unfavourable circumstances, than if made under more favourable circumstances. The only assumptions upon which my criterion for the rejection of anomalous results is based are contained in the above statements.

I assume that a particular person, with definite instrumental means and under given circumstances, is likely to make, on an average, one mistake in the making and registering n observations of a given class. The probability, therefore, that any record of his of this class of observations as a mistake is $\frac{1}{n}$. From the average discordances amongst the registered observations of this class we can find the probable error of an observation in the usual way, and also the probability of an error greater than a given quantity, as C. Then my principle simply amounts to this,—that if the probability in favour of a discordance as large as C is less than that of a mistake, or $\frac{1}{n}$, I am not only justified in rejecting, but, as a reasoning animal, required to reject the discordant observation. I do not pretend that this method affords a principle by which mistakes can be recognized with absolute certainty. It is quite possible, in accordance with my views, that mistakes should occur, too small to be rejected by my criterion. It is quite possible that the result rejected as a mistake may arise from an unusual aggregation of the results of several independent sources of error, each of which has acted upon the other observations. But what I do maintain is (1) that the rejection of a mistake is of far more importance than the omission of an observation from a group. One mistake may destroy the evidence afforded by a considerable group of observations. (2) That the rejection of results which appear amongst the records of observations can only be justified by a consideration of opposite probabilities. (3) That when the evidence in favour of a result arising from the action of the ordinary sources of error is less than that, it arises from sources of error different from those in constant action, and with those results, if I could separate them, I have no concern; then I am not only justified but required to reject such discordant result.

I have never seen any clear objections raised to the principle upon which my criterion is based. I can, however, quite understand that anyone may have serious doubts, before trial, of its practical value. It is no doubt true that the value of n for a

given observer and class of observations can rarely, if ever, be determined with perfect accuracy. If, therefore, an accurate value of n be required in practice, the method would, as a practical guide, fail. But such is not the case. I consider that one of the most important results of my paper was to show that no assumptions could be made respecting the number of observations which, on the average, were made with one mistake, without leading to a limit beyond which there was a greater probability of an error arising from a mistake than from the average run of errors, and that with any suppositions that could be admitted in practice, the limits of rejection differed but slightly. If the probable error of a result is equal to $0''{\cdot}477$, and we assume an observer to commit one mistake in the making and recording 100 observations, I have shown that the limit of rejection would be $1''{\cdot}9$; but if we assume that the observer only commits one mistake in the making and recording of 1,000 observations, the limit of rejection would only be shifted to $2''{\cdot}3$. Nor is this all. If my criterion be accepted, it follows that for a class of observations in which the liability to mistakes remains sensibly constant, whilst the probable errors of the results vary; as for instance, Polar Distances at different Zenith Distances, then the limit of rejection varies directly as the probable error. Thus assuming one mistake in the making and recording of 500 observations, we should have no more grounds for the rejection of a discordance of $17''{\cdot}6$ for a Zenith Distance a little greater than $87°$ than for the rejection of a discordance of $2''{\cdot}2$ at a Zenith Distance about $26°$.

As Mr. Glaisher thinks that "It is pretty clear that it would be a good deal easier to choose a limit of retention at once than to guess a value of n and deduce one," I would ask him to consider the above case and the following. Is it easier to decide for Zenith Distances about $87°$ with probable eror $3''{\cdot}82$, whether the limit of rejection should be $7''{\cdot}6$ or $17''{\cdot}6$, or to decide whether the observer is likely to be so grossly careless as to make one mistake in every five observations, or so careful as to only make one in five hundred? I have very little doubt in which way this question would be decided by most minds. My criterion, if its principle be accepted, shifts the decision of the first question upon the second. Our mathematics, after all, can do no more for us than to bring the decision of complex questions to more simple forms. This, I believe, my criterion effects to a very considerable extent.

There is one point connected with the applications of this criterion to which I should wish to call attention. It is quite conceivable that in certain cases the number of rejections by an application of the criterion would be greater than could be admitted on the supposition of the discordances arising from mere mistakes. Such a case would have arisen in the discussion of Star places before the discovery of aberration, and in all similar cases. Here, if the number of rejections pointed out by

the criterion is greater than can be admitted on the supposition of mere mistakes, we are put, with greater or less certainty, according to the number of cases of rejection pointed out above the number of admissible mistakes, upon the trace of sources of error which act with variable intensity upon the different results or upon the discovery of new laws of nature.

Before offering any remarks upon the method of treatment of observations which Mr. Glaisher advocates, I must call attention to some miscellaneous remarks scattered over his paper, which appear to me to indicate, that, although Mr. Glaisher accepts the results of the ordinary theory, he does not accept the principles upon which alone, so far as I am aware, these results can be proved. For instance, I entirely demur to the principle of Mr. Glaisher's remark—"Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the way contemplated by the theory and independently of the observer's care), and yet their retention may be quite as disastrous as if they had been mistakes made by the observer." The principle of this statement is, in my opinion, entirely opposed to the first principles upon which the law of facility generally adopted is based. I presume, however, that this statement must be connected with another—"The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise, and in that case there is no distinction between continuous and intermittent errors at all."

This is the mere language of the schools, to which an answer, equally satisfactory, might be given in the same language. Although it is true that the continuous errors will not look after themselves until the number of observations is infinite, yet an infinity of a different order is required before the intermittent errors will be found to do so. Practically, however, we have nothing to do with infinities of observations.

It is only so far as we can legitimately assume that the mutual destruction of errors which would take place in an infinity of observations will take place sensibly in the number of observations with which we have to deal in practice, that we can transfer the theorems of the calculus of probabilities to the discussion of the results of observation.

In such cases it is most certainly not true, because we may assume as a close approximation to truth that this mutual destruction of errors takes place for the continuous errors, we may with equal truth assume that it takes place for intermittent errors arising from causes which will act only once in two or three hundred observations, and which, when they do act, may produce errors enormously larger than those arising from the causes in continuous action. These remarks meet, I believe, all Mr. Glaisher's statements respecting my criterion. (*Monthly Notices*, vol. xxxiii. pp. 398 to 400.) We have not, in practice, to deal with such numbers of observations that the average

number of observations with one mistake is a very small fraction compared with the number of observations discussed.

Unless this is the case, Mr. Glaisher's remarks on page 400 have not, in my opinion, the slightest bearing upon the practical aspects of the question. In my paper of 1868 I pointed out what I believe is a valid objection to a criterion proposed by Professor Chauvenet, viz.:—that whenever the probability of an error in n observations was less than $\frac{1}{2}$, the corresponding observation should be rejected.

My objection was thus stated:—"In $2n$ such observations we ought reasonably to expect an error greater than or equal to x. In n observations, therefore, we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." To this Mr. Glaisher appends the following remark:—"Taking the words literally, it is enough to reply that the question is not how far we are surprised at such an error having been made, but whether it should be retained or not." I do not know whether Mr. Glaisher wishes it to be understood that he thinks the objection I have made unsound; but I presume he would hardly have extracted my remarks upon this point and appended his own unless he did think so. If such be the case, I fear that nothing I can say will make the point clearer to him. My objection simply amounts to this—that if the odds are fairly two to one against an event coming off, and yet the event does come off, we have no right to assume from this alone that unfair practices have been resorted to. There is a statement made by Mr. Glaisher (*Monthly Notices*, vol. xxxiii. p. 402), to which I will now call attention:—"Another point deserving notice is the statement, usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems, at first sight, not to be the case; for, suppose one were to estimate the area of a field containing, say, two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero or minus two (a case of this kind was pointed out to me by a pupil in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark." Now, I must confess that I have never heard any one before maintain that positive and negative errors are always equally probable. There are such things as systematic errors recognized in practice, *i.e.* errors which, for an observation made with a particular instrument in a particular way, will always enter into the result with the same sign. I believe that it is usually assumed that it is only for those classes of errors for which positive and negative results are equally probable that theorems which depend upon the ordinary law of facility of errors can be appealed to as means of the elimination of error in the result.

But this can hardly be Mr. Glaisher's meaning, for such a view would have prevented the difficulty mentioned from being raised.

Assuming the law of facility

$$\frac{h}{\sqrt{\pi}} \cdot \varepsilon^{-h^2 x^2} dx$$

for an error between x and $x + dx$

and denoting the n independent results by a_1 , a_2 . . . a_n; $a_1 - x$, $a_2 - x$, &c., by x_1 , x_2 . . . x_n, and the corresponding measures of precision by h_1 , h_2 . . . h_n, we have for the probability u of the coexistence of the errors x_1 , x_2 . . . x_n

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

Then assuming $h_1 = h_2 \ldots = h_n = h$ a constant, Mr. Glaisher deduces, in accordance with the ordinary theory,

$$x = \frac{a_1 + a_2 \ldots + a_n}{n}$$

With this result, the probabilities in favour of x_1 , x_2 . . . x_n are computed.

Mr. Glaisher then proceeds to obtain a result which he considers more probable than the arithmetical mean, as follows. Having found the probabilities (on the assumption made, viz. :— that $h_1 = h_2 \ldots = h_n$ in favour of errors x_1 , x_2 . . . x_n to be p_1 , p_2 . . . p_n he weights the results in accordance with the probabilities, and determines x from the condition that it shall make a maximum of

$$\varepsilon^{-[p_1^2 (a_1 - x)^2 + p_2^2 (a_2 - x)^2 + \ldots]}$$

This value of x is considered as more probable than the arithmetical mean, and Mr. Glaisher recommends that the process should be continued until the consecutive values of x come out sensibly the same.

I admit that whenever a large discordance from the arithmetical mean is an indication of the action of sources of error of an abnormal character, less weight should be given to the corresponding observation; but instead of regarding Mr. Glaisher's method as mathematically complete, I regard it as mathematically unsound, even upon the assumptions made.

If we are to ignore all *a priori* considerations, and to adopt as the most probable result the value of x which makes

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

a maximum, where h_1 , h_2 . . . h_n are to be considered, as Mr. Glaisher supposes, unknown quantities whose values are to be assigned from a consideration of the mere run of the numbers in the question before us. Then clearly h_1 , h_2 . . . h_n are

variables, and must be treated as variables throughout the mathematical process for finding the maximum value of u.

Differentiating u and equating the differential to zero, we have

$$\frac{Du}{u} = 0 = 2\,(h_1^2x_1 + h_2^2x_2 + h_3^2x_3 + \ldots)\,dx$$

$$+ dh_1\left(\frac{1}{h_1} - 2\,h_1x^2{}_1\right) + dh_2\left(\frac{1}{h_2} - 2\,h_2x_2^2\right) + \text{\&c.}$$

$$\therefore h_1^2x_1^2 = h_2^2x_2^2 = h_3^2x_3^2, = \text{\&c.} = \tfrac{1}{2}$$

and $$h_1^2x_1 + h_2^2x_2 \ldots + h_n^2x_n = 0$$

or $$x = \frac{h_1^2a_1 + h_2^2a_2 + \ldots}{h_1^2 + h_2^2 + h_3^2 + \ldots}$$

From which we deduce for the determination of x the equation

$$\frac{1}{x_1} + \frac{1}{x_2} + \frac{1}{x_3} + \ldots + \frac{1}{x_n} = 0 \quad (1)$$

The value of x which makes u an absolute maximum must be contained amongst the roots of the equation (1), and this equation will always have one real root which will make u a maximum.

Such is, I believe, the correct solution of the question on the assumptions made, that the weights of each observation are to be deduced entirely from the run of the observations under consideration without any regard to *a priori* probability.

On the Limit of a Possible Lunar Atmosphere.

By E. Neison, Esq.

In a preliminary note in the *Monthly Notices* of the Royal Astronomical Society (Vol. xxxiii. p. 464), attention was drawn to the circumstance that no proof of the absence of a sensible lunar atmosphere had as yet been offered, but only that it did not exceed a certain undecided limit. The question possessing no little interest, and being of importance with reference to the present constitution of the Moon's surface, it appeared desirable to fix the limit as satisfactorily as possible within which a lunar atmosphere was possible. For there can be no doubt but that any atmospheric envelope to the Moon, however small in density, must exert a considerable influence on the surface, and enable actions to take place otherwise impossible.

There can be no question but that the main circumstance limiting the density of any possible atmosphere is the refraction of the rays of light it would cause. From what we know on the subject, as there can be no doubt but that it must be of comparatively small density, it is evident we have only to deal with the hori-

zontal refraction, as the refraction must practically vanish for any beyond very low altitudes.

For the purpose then of ascertaining the limiting value for any possible lunar atmosphere, it will be requisite to determine within what limits there may be a horizontal refraction. Taking everything into consideration, the process best adapted to detect this horizontal refraction would be the different values given by a telescopic and occultation determination of the semi-diameter of the Moon. After having determined to within what value the horizontal refraction is limited by the known values of the lunar semi-diameters determined by these means, it would be possible to ascertain if any other phenomena still further limits it.

The Astronomer Royal has shown (*M. N.* xxv. p. 261, *Greenwich Observations*, 1864, Appendix I.,) as the result of the computations by Mr. Breen of the Moon's occultation semi-diameter, from observations of 295 occultations, that the following corrections are given by it to the Greenwich telescopic semi-diameter:—

By disappearance of stars at the dark limb	=	$-2''{\cdot}00$
By reappearance of stars at the dark limb	=	$-2\ {\cdot}40$

The similar observations at the bright limb give greater values for the semi-diameter, as might *a priori* be expected, both from the extreme delicacy and difficulty of the observations, and from the irradiation of the limb being apt to extinguish the light of the star before it reaches the actual limb. As Sir George Airy remarks: "We cannot be sensibly in error in saying that the Moon's occultation semi-diameter is less than the Moon's telescopic semi-diameter by $2''{\cdot}0$."

This excess is generally attributed to the results of irradiation at the bright limb increasing the telescopic semi-diameter, as the Greenwich value for this, referred to above, is considered a very exact determination; and although this may be the correct explanation, there can likewise be no doubt but that a portion, if not the whole, may possibly arise from a lunar atmosphere. Hansen's value for the semi-diameter of the Moon is regarded as being an exact determination of the true diameter, and is considerably less than the Greenwich telescopic semi-diameter; a reduction, however, of the occultation of stars for the years 1861 to 1870, gives a correction for the most favourable and accurate class of phenomena of $-1''{\cdot}70$ to this value, and the greater portion of this correction it appears cannot arise from the effect of irradiation. As a certain amount of irradiation must occur, and can but affect the telescopic semi-diameter, we cannot assume the whole correction to the telescopic semi-diameter as being due to the possible existence of a lunar atmosphere; and it would appear more satisfactory to allow for this, and say that these results show that an atmosphere may exist on the Moon capable of exerting a horizontal refraction of about one second of arc. Apart from other evidence, the observa-

tions of solar eclipses; especially those of 1860 and 1870 at Greenwich, where we have the effect of irradiation at its maximum, and exerted in diminishing the apparent lunar diameter; show that it is impracticable to ascribe to irradiation the excess of observed and computed semi-diameters over the occultation semi-diameter, as they unite in giving a maximum correction to Hansen's semi-diameter from the effects of irradiation of $-0''.5$.

It remains now to determine on the basis of a possible horizontal refraction of one second of arc, the probable condition of the supposed lunar atmosphere. For the purpose of computing the horizontal refraction it will only be necessary to ascertain a probable law of decrease of density, as it has been shown very different rates of decrease give sensibly the same horizontal refraction, and considering the smallness of the horizontal refraction to be determined, it will be evident that considerable errors on the Earth would be insensible upon the Moon. As the rate of decrease of density depends upon the rate of decrease of temperature, and as there is no possibility of determining this, it will be necessary to assume some hypothesis. Reasoning from analogy, and taking into consideration the much slower decrease in density from the action of gravity, and the greater mobility from the decrease in density, it would appear we must have upon the Moon, not only a more equable decrease in density, but a greater uniformity in temperature, and consequently a slower decrease. It will be assumed that near the surface the decrease of temperature for increasing heights will be nearly equable, and slowly decreasing to a definite temperature at the upper limit of the atmosphere, an hypothesis similar to that of Laplace for the Earth. (*Mécanique Céleste*, tome x. ch. 1, § 7.) It may here be remarked that the more rapid the decrease of temperature the less the horizontal refraction becomes.

For the purpose of determining the refraction, it will be assumed that the atmosphere is unlimited in height, and that as before stated the temperature decreases slowly to a fixed minimum as the altitude increases. Unlike the Earth, the Moon suffers great variations in the temperature of its surface. Lord Rosse's researches show the temperature of the surface of the Moon to be very variable, reaching probably a maximum temperature of nearly two hundred degrees centigrade, and falling probably considerably below zero. This last is readily seen, considering the long lunar night, and the slight retarding influence exerted by a rare atmosphere free from moisture. As the temperature of space must be constant, it is evident this difference in the surface temperature will materially alter the constitution of the atmosphere and the amount of the refraction. We cannot from Lord Rosse's figures deduce any law for determining the variation of temperature, but it may approximately be taken as varying as the sine of the solar altitude. Consequently the maximum surface temperature of the following limb, would be at about the eighth day of the Moon's age, and the minimum about two days before full, while for the

other limb the similar periods would be the day after the third quarter and two days before full.

For the temperature of space, and therefore of the upper portion of the atmosphere, we have according to Fourier and Hopkins, about — 50° centigrade, which value is adopted; but it is evident that by lowering this value we have not only a less horizontal refraction, but a greater mass of atmosphere, and therefore even more favourable conditions than those assumed. It is true by taking the temperature near — 266° centigrade it renders the determination of the height of the atmosphere easier; but we have no reasons for assuming this to be in any sense probable; and Ivory has shown the height of the atmosphere does not sensibly alter the refraction. Upon, then, this value for the temperature of space we have for the minimum surface temperature of the Moon — 30° cent. We must refer to one or two points before proceeding further. In considering the conditions of the atmosphere, the first mile from the surface is excluded, as liable to great local variations, and as not taking part in the refraction. This last is apparent, as for one hundred miles in width at the limb, the numerous mountains undergo no sensible diminution in height, and so must form a projecting ridge of quite this height. To determine the height of the atmosphere, as Laplace has shown that the height is of necessity limited, we must take the following consideration; for although to determine the refraction we may take the heights as unlimited, for other purposes we must determine its probable limit. We have physical reasons for believing that after the decrease of density has reached a certain amount, the elasticity of the atmosphere is destroyed at a medium low temperature, and this we must suppose reached at the limit of our own atmosphere. By taking then the density here as the minimum density, we can fix approximately the height of the lunar atmosphere as the point where this density is reached. Various heights for our atmosphere have been fixed, at from twenty-five to eighty miles, and by taking one hundred miles, it may be regarded as the extreme. Calculating upon Laplace's theory the density of the atmosphere at this height, we have, taking the surface density as unity, a number whose common logarithm is $\bar{1}2{\cdot}3$. As the Moon's atmosphere will be about one five-hundredths of the density of that of the Earth at the surface; we may take the Moon's atmosphere as ceasing when its density is but the number whose logarithm is $\bar{9}{\cdot}0$ of the surface density. It is evident, however, that variation in this will exert very slight effect upon the density at the surface.

The physical conditions of an atmosphere upon the hypotheses adopted have been investigated by Laplace, Ivory, and Plana; but that of Ivory has been selected as not only generally preferred, but as from its simplicity admitting of being readily modified so as to suit conditions found on the Moon. Ivory's Memoirs are contained in the *Philosophical Transactions* for 1823 and 1838, and simple integrations of his equations are given by

Brünnow (*Spherical Astronomy*), and Bruhns (*Astronomische Strahlenbrechung*, pp. 148–159); they can also be readily deduced from Laplace's equations (*Mécanique Céleste*, tome x. ch. 1, § 7), and to one of these reference must be made for the demonstration of the equations used.

The notation used is as follows:—

a = radius of Moon.

x = altitude above surface of any point P.

δ, p, t = density, pressure, and temperature at P, and δ_o, p_o, t_o ditto at surface.

l_o = height of column of air of density δ_o, that at temperature t_o would exert under the action of the Moon's gravity, a pressure equal to p_o.

$\alpha = 0{\cdot}000294\delta_o'$, putting this last in terms of the density of air at the surface of the Earth, and at 0° cent. and 760 millimetres pressure.

$\epsilon = 0{\cdot}003665$, its usual value as determined by Regnault.

Remembering that the height is to be considered unlimited, and as upon the hypotheses taken where m varies as the height of the atmosphere,

$$\frac{\delta}{\delta_o} = \left(1 - \frac{u}{m+1}\right)^m$$

we have for m infinite.

$$\frac{\delta}{\delta_o} = e^{1-u},$$

and

$$\frac{p}{p_o} = e^{-u}\left(1 - f + fe^{-u}\right), \text{ and } \frac{p\,\delta_o}{p_o\,\delta} = 1 - f\left(1 - e^{-u}\right) = \frac{1+\epsilon t}{1+\epsilon t_o}$$

where

$$x = l_o\left\{(1-f)\,u + 2f\left(1 - e^{-u}\right)\right\}.$$

where f is the function on the value given to which depends the rate at which the temperature decreases. It is evident from the third of these that as f increases the rate of decrease of temperature decreases, and that for $f = 0$ we have a uniform temperature, and that it cannot exceed unity.

Finally, for the horizontal refraction we have, putting r for the refraction and θ for zenith distance,

$$dr = \alpha(1+\alpha)\sin\theta\left\{\frac{d\left(1 - \frac{\delta}{\delta_o}\right)}{\sqrt{\left\{\cos^2\theta + 2\frac{x}{a} - 2\alpha + 2\alpha\frac{\delta}{\delta_o}\right\}}}\right\}$$

which equation can be expanded into terms of the well-known form $e^{T^2}\int_{T}^{\infty} e^{-t^t} dt$, which can be readily integrated and reduced to the approximate form as under, with only insensible errors.

$$r = a(1+a)\,\frac{\sqrt{\pi\,\frac{a}{l_o}}}{\sqrt{2}}\left\{1 - \left(f - a\,\frac{a}{l_o}\right)\left(\sqrt{2}\cdot\ 1\right) + f\left(\frac{3}{2} - \sqrt{2}\right)\right\}$$

The term f can readily be deduced from the fact that it must be such as to make the temperature equal to that of space at the upper limit of the atmosphere, and not affect the surface temperature; and as from one of the equations above, we have $f = 1 - \frac{1 + \varepsilon t}{1 + \varepsilon t_o}$ approximately, by putting for t and t_o, the temperatures of the upper and lower limit of the atmosphere respectively, f is at once determined. It is also apparent that for the upper limit of the atmosphere, taking it as fixed by the value for the density before fixed, u will be approximately twenty, or the height of the atmosphere, for all surface temperatures remain sensibly constant, at a height slightly above half the Moon's radius.

Now, determining the surface density, so that the horizontal refraction at 25° centigrade will not exceed one second of arc, and be a convenient number for calculation: this will be found best represented by 0·0025 of the Earth's surface density at its standard pressure and temperature, or 760 millimetres and 0° centigrade. Putting then these values in the formulæ for the horizontal refraction, we have it amount to the following for the surface temperature stated:—

Surface temperature.		Horizontal refraction.		
−30°	centigrade	...	1″·27	Mean temperature of Dark Limb.
0°	,,	...	1·03	
25°	,,	...	0·88	
100°	,,	...	0·59	Mean temperature of Bright Limb.
200°	,,	...	0·39	

It will be seen that these results are confirmatory of the cause of the difference between the occultation and telescopic semi-diameters, having their origin in the presence of a lunar atmosphere.

We have taken with Sir John Herschel the actual retardation at the limb, at its generally received value, namely, the horizontal refraction; and not twice this, as has been done. It can readily be shown that the retardation of an occultation is less than twice the horizontal refraction, as follows:—Although the angle between

the paths of a ray before and after refraction is evidently twice the horizontal refraction, yet it is apparent that the ray that would reach the observer is not that which would reach him in the absence of an atmosphere, but one lying much further from the limb; while that which would otherwise reach the observer after grazing the limb in the absence of a lunar atmosphere, would, if it were present, strike the surface of the Moon, and not reach the observer.

It remains now to notice the objections that have been raised to the existence of a lunar atmosphere, and it is evident that with one or two exceptions, as they were all directed against an atmosphere usually as dense, or even denser than our own, they are valueless as directed against one only one four-hundredths of this density. The phenomenon referred to by Mr. Proctor, in his Work on the Moon, as preventing the occultation of a star, could only arise from a lunar atmosphere much greater than our own, even were it not prevented from the rays from the Moon after refraction being divergent and not convergent, as he assumes in his illustration. It will also be apparent that for the density of the supposed atmosphere, no distortion of a star before occultation could possibly occur, and the same applies to the occultation of a planet such as *Jupiter* or *Saturn*, the maximum effect would be to increase the size of the planet by about one-thousandth; but in no case distort it. Dr. Huggins' observation (*M. N.* vol. xxv. p. 60) is evidently by no means delicate enough to detect the very slight effect capable of being exerted by an atmosphere of the density supposed. The effect of a lunar atmosphere upon an eclipse of the Sun, would, if of the density assumed, be sensibly the same as a diminution of the semi-diameter by about one second, or would be lost in the effects of irradiation. Finally, it can hardly be seriously urged that it could materially interfere with the observation of the reversal of the dark lines in the solar spectrum, considering the smallness of the horizontal refraction, and the extremely minute amount of scattering of the solar rays the supposed atmosphere could effect. No known objection yet raised appears to limit a possible lunar atmosphere more than the difference between the occultation and telescopic semi-diameter.

The real dimensions of the atmosphere shown to be possible upon the Moon's surface, can be best shown by the fact that its total weight above one square mile is about four hundred thousand tons; and that it bears nearly one eighth of the proportion of the Moon's mass, as the Earth's atmosphere does to the Earth's mass. The consideration of these features, however, had better be deferred until the fact of the presence of an atmosphere has been demonstrated.

In conclusion it is to be observed that the purport of the present paper has been to show that it is *possible* that a lunar atmosphere *may* exist, and to define its probable condition; the task of showing that a lunar atmosphere *does* exist requires different and more certain evidence.

On an alleged Variability of the Sun's Diameter. By Dr. Auwers.*

(*Abstract by W. T. Lynn, B.A.*)

Lindenau found in reducing the observations of the Sun with the transit instrument at Seeberg, in the years 1808 and 1809, differences in the observed diameters which he thought could not be explained by errors of observation, especially as they seemed to be of a periodic nature. By a discussion of the meridional observations at Greenwich from 1750 to 1755, and from 1765 to 1786, he obtained an apparently complete confirmation of the reality of these variations and their periodical character, and was able satisfactorily to represent the observations by the hypothesis that the Sun was an ellipsoid rotating above its major axis; the compression, according to his calculation, amounting to something between $\frac{1}{279}$ and $\frac{1}{140}$.

This was communicated to the number of Zach's *Monatliche Correspondenz* for June 1809. In the following number of that periodical, Bessel remarked that the variations noticed in the Sun's apparent diameter at Greenwich could be explained by a periodical shifting of the wire-frame of the instrument in reference to the plane of the focus. Lindenau's investigation has not led to any further criticism; more accurate observations showing no deviation in the Sun's apparent disc from that of a circle, and his theory of a compression of the Sun's body was set aside as resulting from errors in the early observations.

Recently, however, Father Secchi, apparently unaware of this investigation of Lindenau, has conjectured that the effect of the active forces in the Sun, which are made known to us by the variable formations on its surface, may produce changes of volume in the masses of luminous gas, perhaps perceptible in accurate observations of the Sun's diameter. He, in consequence, caused his assistant at the Observatory of the Collegio Romano, P. Rosa, to make regular observations from July 1871, of the duration of the Sun's transit with the meridian-circle; the result of which appeared to confirm his suspicion of considerable variations in the Sun's diameter, and a close connection of this with the indications of the force forming the spots and protuberances. The variations in the apparent diameter were greatest when the activity of the forces was greatest; and (as Secchi asserted) these variations often exceeding 3″ in amount were greater than, and could not be explained by, mere errors of observation. Their sometimes regular change also seemed to negative the latter idea; and at Secchi's instance, observations were afterwards made at the Palermo Observatory, which appeared to confirm the result obtained at Rome. They agreed in showing that "the greater diameter was observed at those times at which the number of the spots and protuberances was less."

* From the *Monatsbericht* of the Royal Academy of Sciences at Berlin for May 1873.

Secchi proceeded to compare the Roman observations (187 in number) with the heliographical latitudes of their end-points, which, during the year (1871 July to 1872 July) of observation, passed four times through the values from 0 to $\pm$ 26°. The curve formed from the observed values thus obtained appeared to him to be regular and confirmatory of each other in the four divisions they contained. He assumed therefore "a maximum of diameter between the equator and $\pm$ 6° of heliographical latitude, its amount being 32′3″·74 and a minimum between $\pm$ 21° and $\pm$ 23° of latitude, the amount being 32′2″·18; the difference of the two being 1″·56, five times as great as the probable error of a single observation, whilst the value of the maximum was established by 31, and that of the minimum by 22, observations."

While Secchi does not attempt to claim these results, founded on only one year's observations, as final, he yet maintains them to be undeniable deductions from the observations made. Dr. Auwers, however, asserts that the foundations of his theory are so unreliable that the consequences deduced from it must be, for the present at any rate, absolutely rejected. In the first place, it is in opposition to all experience in the nature of errors of observation, to make the deviations of the separate observed transits from their mean the only measure of the possible errors. That greater fluctuations show themselves in the observed diameters than would correspond to these deviations can, up to a certain limit, only be regarded as a proof that the determination of the probable error of observation is incomplete; and that this limit has very probably not actually been exceeded by the fluctuations in the Roman observations is shown by an investigation,* by Herr Wagner, of Pulkowa, of a series of observations evidently surpassing those at Rome in their internal goodness. The observation of a transit of the Sun's limbs is affected to such an extent in this instance, by an element totally left out of account by Secchi,—namely, the state of the atmosphere and the quality of the image produced by its condition—that differences occur between means from repeated observations of the Sun's diameter made with the great transit instrument at Pulkowa, in good, and others in bad. states of air, of from 3″ to 4″, so that differences of from 5″ to 6″ between separate observations cannot certainly be regarded as anything surprising. The data for estimating the actual amount of influence produced in the Roman observations by the condition of the atmosphere are not furnished by Secchi, and it cannot, therefore, be decided how far this element is capable of explaining the observed fluctuations; but this circumstance is sufficient to overthrow the proof which he has deduced from those observations.

With regard to the alleged agreement between the Rome and Palermo observations, Dr. Auwers demurs to the fact; it appears,

* Published in the 1873 January part of the *Vierteljahrsschrift der Astronomischen Gesellschaft.*

indeed, that although both series assign a principal minimum on April 1872, yet a detailed examination shows that the general agreement of the deviations is scarcely more marked than the contrary. And as to the third proof of the reality of the variations observed at Rome, viz. their alleged connection with the visible fluctuations of intensity in the activity of the forces on the Sun, Dr. Auwers states that he can find no satisfactory data in the published observations for the somewhat arbitrary tabulation of Secchi, or the conclusions deduced from it. He proceeds to subject these conclusions generally to the test of a comparison with more comprehensive materials. This enquiry, he remarks, is not without an obvious importance in the question of the relative accuracy of the different methods of observation to be employed in the transit of *Venus*. For observations of which he has availed himself for this purpose, which have been made, but not yet published, at Greenwich, Neuchâtel, Oxford, Washington, Paris, Königsberg, Brussels—all taken during the same period of time as those at Rome—Dr. Auwers offers his thanks to Sir George Airy, Messrs. Becker, Main, Sands, Loewy, E. Luther, and Quetelet.

From the whole mass of observations, extending from July 1871 to July 1872, "it clearly results," says Dr. Auwers, "that Secchi's assertions concerning variations in the Sun's diameter are totally and entirely unfounded, and that the changes noticed by him are due to casual errors of observation." Of course, this conclusion is only meant to imply "that changes in the diameter, produced by such variations of activity in the superficial strata, as occur within one tenth of a solar-spot period (or generally any changes within a shorter period than this), are smaller than can be recognized within that period of time by meridional observations. The possibility remains of being hereafter able to perceive such changes, either by more delicate observations or by comparisons through longer intervals of time, such as whole sun-spot periods."

Dr. Auwers examined some heliometer observations of the Sun; but the number available is not great, and no conclusion of certainty can be derived from them. He has also put together a long series of Greenwich observations by Bradley and Maskelyne and their assistants; and some shorter ones by Bessel at Königsberg, and by W. Struve at Dorpat, with the view of ascertaining whether there is any observable connection between the Sun's apparent diameter and the state of the solar-spot period; but he does not find it possible to trace any such connection. Finally, he has compared the values of the Sun's diameter given in the last twenty years in the Greenwich Introductions (from 1851 to 1870) deduced from the Transit-Circle observations, which again show, when compared with Wolf's relative number of the solar-spot condition, "that in the fluctuations of the observed values, both for the horizontal and vertical diameters, and the difference between the two, no dependence upon the variations of the degree of activity, and, therefore, no indication whatever of the reality of these fluctuations can be perceived."

On the Variable Proper Motion of Procyon. By Dr. Auwers.

(*Abstract* by W. T. Lynn, B.A.*)

The discovery by M. Otto Struve of a small companion to *Procyon*,† which might account for the motion of that star around its common centre of gravity with some unknown body, has induced Dr. Auwers to re-determine the motion of *Procyon* by the aid of the more recent observations, with a view of ascertaining whether this were so.

In doing this he has availed himself of observations at Greenwich, Edinburgh, Cambridge, Oxford, Paris, Brussels, Washington, Williamstown, Melbourne, Santiago, Cape of Good Hope, Geneva, and Leyden. He thus arrives at a set of elements of the orbit of *Procyon* not greatly different from that which he obtained before, the period being 39·866 years; and this represents very well the whole of the observations from 1752 (Greenwich) to the present time. It confirms him in his theory of a motion of the star in a nearly circular orbit in the plane of projection in the sky, or perpendicular to the line of sight.

With regard to the object observed by M. Struve being the disturbing cause, he remarks that the angle of position of such a hypothetical companion at the time of observation (1873·24) would be 70°·8, while that which M. Struve actually observed was 87°·6. "The difference of 16°·8 (corresponding, at the place of *Procyon*, to 0″·29) is about four times its probable error, so that the assumption of the identity of this observed companion with the theoretical one is not of the most favourable kind, though the contrary is by no means proved." He goes on to say that the next spring will afford an opportunity of deciding this question, if it is possible then to observe Struve's companion-object again. For if that object is really the disturbing cause of the observed motion of *Procyon*, its position-angle at the end of March 1874 will be about 97°, whereas if it is only optically close to *Procyon*, the position-angle will be only 84°.

If the former appears to be the case, it will be necessary to attribute to *Procyon* the greatest mass hitherto known, amounting in fact to 80 times that of the Sun, whilst that of the companion would be concluded to be equal to seven masses of the Sun—the parallax being taken at 0″·24. For M. Struve's object was observed by him at a distance of 12″·4 from *Procyon*; and it follows that if it be the cause which produces the nearly circular motion of that body, it must be placed at a considerable distance from the common centre of gravity of the two.

This new and improved determination of the elements of

* From the *Monatsbericht* of the Royal Academy of Sciences at Berlin for 1873, May.

† See *Monthly Notices*, vol. xxxiii. p. 430.

the motion of *Procyon* by Dr. Auwers gives the following results:—

Epoch of Minimum R.A.	1795·629 ± 0·675 years
Annual Motion	9°·02993 ± 0°·11966
Periodic Time	39·866 ± 0·528 years
Radius of Orbit	0″·9805 ± 0″·0375

On a remarkable Nebulous Spot observed upon the Sun's Disc by Pastorff, May 26th, 1828. By A. Cowper Ranyard, Esq.

In the first volume of the MS. Observations of Geheimerath Pastorff, which were presented to the Astronomical Society by Sir John Herschel, is a drawing of a dusky spot, with a bright nucleus, seen upon the Sun's disc, on the 26th of June, 1819, and which has been generally believed to have been the comet of 1819, as seen projected upon the bright background of the photosphere. The object of the following note is to draw the attention of the Society to a very similar nebulous spot, with a small bright centre, depicted upon a drawing of the Sun's disc for the 26th of May, 1828. It is marked by Pastorff as "Hell glänzender Fleck" (bright shining spot), and its distance parallel to the horizon from the nearest point of the Sun's western limb is given as 3′; the observation was made at half-past nine in the morning, at Buchholz,* near Drossen. On the drawings of the Sun's disc for the preceding and succeeding days no such spot is registered, nor is any sun-spot or facula given as in or near its place; and, indeed, the faint nebulous haze with a minute bright centre given here, and on the drawings for the 26th of June 1819, differs totally from the type of drawings given all through the MSS. to represent sun-spots or faculæ. It would therefore be interesting to enquire whether any known comet or meteoric stream lay between us and the Sun on the 26th of May, 1828. It should be remarked that the drawings for May 1828 are on a much larger scale than those for 1819, and that the nebulous spot is both smaller and fainter than in the earlier drawing; it is therefore probable that the comet (if such it be) will turn out to be a very small one compared with that of 1819.

Remarks on Spectroscopic Observations of the Sun, made at the Temple Observatory, Rugby School, in 1871-2-3. By J. M. Wilson, Esq., M.A., and G. M. Seabroke, Esq.

During the last two years and a quarter observations have been made at the Temple Observatory, Rugby, on the positions and forms of the solar prominences, with as much regularity as

* Lat. 52° 25′ 49″ N.; Long. 50ᵐ 32ˢ·17 E. of Paris.

weather permitted. The observations were made from August 1871, to December 1872, with the straight slit, in the usual manner, and since December 1872, with the annular slit, described by one of us, in conjunction with Mr. Lockyer, in the *Proceedings* of the Royal Society in January 1873. This method is found to be satisfactory, and to save much time.

The principal result of these observations is to show, in a very decided manner, that the solar prominences preponderate greatly in the equatorial region of the Sun; that is, they occupy the same region as the spots.

We propose therefore to offer some brief remarks on a theory of prominences and spots, intended primarily to explain their joint occurrence, but incidentally accounting for other phenomena both of spots and prominences. These phenomena are—

(1.) The travelling of spots on the Sun's disk in the direction of his rotation with a velocity increasing as the spots near the equator.

(2.) The rotation of spots.

(3.) The appearance of the penumbra as the spot nears the edge.

(4.) Spectroscopic observations on spots.

(5.) Connection of faculæ and spots.

Our reason for publishing such a speculation is that it suggests further observations, some of which require higher telescopic power, and more leisure, than we have at our command.

Let it be supposed (1) that at some depth below the Sun's surface the pressure is such that substances such as some metals are in a liquid state, though at a temperature far exceeding their boiling point under ordinary pressure. This will be the case unless the temperature of the interior of the Sun is above all their critical temperatures, in which case no pressure would liquify them; and the fact of some liquids or solids existing in the Sun seems probable from a consideration of its density.

In this case, the liquids will on the whole arrange themselves in order of density, the denser being below the lighter.

Such liquids maintain their liquid state solely in virtue of pressure, and if that is diminished they may burst into vapour. The equilibrium therefore must be unstable; a disturbance of pressure will create an outburst, originating at whatever depth liquids exist in this condition, and the outburst will travel in the direction of least resistance, i.e. towards the surface.

This result will be remarkably intensified if (2) there exist two liquids in the Sun so related, that the denser has the lower boiling point, and not capable of freely mixing with each other, like water and oil, or like chloroform and water. The effect of this combination is easily studied by taking a long glass tube, closed at the bottom, and having nearly filled it with water, pouring a few drops of chloroform into the water. If then the water is heated to near its boiling point by a flame held to the side of the tube above the chloroform, and then the chloroform

itself is heated, the water is violently ejected from the tube, the vapour of chloroform being superheated in the boiling water. Such a combination makes a kind of geyser of very sudden and violent action.

On these hypotheses, disturbances of pressure will produce vertical eruptions through the upper layers of the Sun, and throw up the chromosphere, and portions of the photosphere, into jets such as are observed. These jets will tend to be cyclonic, as we and others have observed them to be, when they cover a large area. These substances, which are carried up into the prominences, will of course be detected, as they have been, by the spectroscope. Further, if liquids or solids are carried up, there will be streaks or flashes of continuous spectrum, as seen, we believe, by Zöllner and Secchi.

Whenever there has been such an outburst, the substances must again fall to the Sun, cooled by the expansion. They will fall, and as they fall, they will produce a region of general absorption and of greater selective absorption. When any portion of the descending material is liquid—like the steam of the geyser eruption descending in water drops—the general absorption will be great; and this will be in the centre of the cooler region of descending material. Moreover, the descent of such a liquid on the heated liquids below would tend to cool those liquids, and cause less emission of light. The chloroform, to recur to the experiment above, descends in drops on the boiling water, and is again evaporated, cooling it each time it falls.

The descent from an elevated region, where the ejected materials will have received an impulse in the direction of the Sun's rotation from the atmosphere through which they will have passed, will cause the descending materials to retain some of that velocity, and thus to travel over the Sun's surface in the direction of its rotation. And in such a descending column rotation would spring up and the spot would seem to rotate.

When a spot is viewed vertically the penumbra, the region of cooler gases, will surround the umbra, the region of cooler gases and liquids; but when seen near the edge of the Sun, the umbra will be lost sight of, as lying deeper than the penumbra.

That the penumbra and umbra should have every degree of distinctness from one another, from the perfect blending, as shown in some of the Harvard sketches, to the sharp separation as usually seen with low powers, is quite compatible with this theory.

The bright bridges and the projecting tongues of light over the penumbra and umbra are portions of the photosphere drawn into the vortex of descending vapour. Or they may be sometimes secondary discharges of portions of the chromosphere thrown up in the neighbourhood of the spot. This would seem to have been the case on March 13 of this year. One of us examined the spot at 11 A.M., and reported in our notebook, 'Hydrogen seen over spot. F and C lines reversed at 11 A.M. half across

spot. The spot was then only partly divided by bridge (*a*) in diagram. Bridge approaching us.' The other of us happened to examine the same spot independently at 11.50, and reported, 'Hydrogen lines seen reversed across the whole spot. The dark line C was reversed, finely defined on the red side, and extending beyond the C, and indefinite on the yellow side.'

The faculæ are elevated regions, and regions in general of ascent. Prominences and faculæ have the same general cause, and will co-exist, the difference between them being simply one of intensity and of localisation to a smaller area. The spot is the region of descent corresponding to regions of ascent, and will therefore occupy the same belts of surface as prominences.

If this theory be true, the following consequences ought to follow from it, and, therefore, to be observed :—

(1.) The period of maximum sun-spots ought to be identical with that of maximum prominences. The prominences have scarcely been observed long enough to establish whether this is or is not true.

(2.) There ought to be a diminution of the height of the chromosphere in the period of maximum sun-spots. This, we have some reason to believe, is the case.

(3.) In maximum sun-spot periods, the umbra of spots should be larger relatively to the penumbra than in the periods of minimum sun-spots.

(4.) Prominences ought, on the whole, to lean in the direction opposite to that of the rotation of the Sun. From our position, relatively to the solar equator, we can only see this, if it is the case, in spring and autumn ; and an examination of all our drawings establishes that this is nearly always the case. The exceptions are very decided lateral jets, which appear to slope indifferently in both directions.

(5.) That faculæ should have a proper motion in the opposite direction to that of a spot.

(6.) That a spot should be more frequently bridged from the West, as we see the Sun, than from the East.

(7.) That there ought to be regions, even near the equator, relatively free from spots for long periods of time; for when disturbance has once begun, it will be long continued.

We do not know whether this could be shown from observation.

(8.) The period of spots ought to coincide with that of some assignable cause of disturbance of pressure.

Rugby, 1873, *Nov.* 11.

On the Determination of Time from Sextant Observations.

By William Lassell, Esq.

It is of late but seldom that I have the pleasure of sending to the Society a communication—indeed I have for some time

ceased to make regular observations with the two-foot equatorial, and what I now do in astronomy is of a very desultory and occasional kind. However, having nearly ceased to observe with a large instrument, I have taken, in this kind of way, to a very small one, and in another department of astronomy—namely, the determination of time.

Since I sent home the Sheepshanks thirty-inch transit instrument, the loan of which I had been permitted to enjoy for some years, I have felt sometimes at a loss for the means of regulating and ascertaining the errors of my clocks—though I had no especial reason for ascertaining their errors within a very small quantity, such as a second or two. But I used many years ago to observe, for determination of time, and occasionally of latitude, with various sextants and circles from six to ten inches radius; and I always found the occupation an interesting one.

The only instrument of this kind I now possess is a small sextant, of only three inches radius, which I purchased from the late Mr. Dollond more than forty years ago. I think he said he had made it for Mr. Davies Gilbert, who for some cause or other had not retained it. The sextant packs in a square box 4·25 inches in the side, and 2·4 inches deep, inside measure. It has a ball-and-socket joint, affixed to a sliding tube, which being screwed into the lid of the box, forms a stand. The telescope, at focus, is 3·4 inches long; and, though it has a clear aperture of only four tenths of an inch, it has two powers, magnifying respectively six and eleven times, defining admirably with the latter, which has quite sufficient light for stars of the second magnitude. The box also contained, when I received it, what professed to be an artificial horizon, being a circular disk about two inches in diameter, of parallel plate-glass, placed in a box-wood cup, to be filled with mercury, the glass disk intended to float horizontally and centrally upon it. But the disk never would remain central; and, though Mr. Dollond introduced four equidistant needle-points to keep it in the centre, it would always be in contact with one or other of the needle-points, thus vitiating its horizontality. I therefore very soon laid it aside.

I afterwards cast and polished a plane speculum, of about the same diameter, mounting it in a brass ring, supported by three equidistant adjusting screws with very fine threads. For levelling this I applied again to Mr. Dollond, and he furnished me with a glass level, the tube about three inches long, and, to quote the inscription upon it—"Ground parallel to show seconds;" which means that one part of its circumference was ground flat throughout its whole length, and parallel to a tangent to the middle of the internal upper surface of the tube; so that, when placed upon a horizontal surface, the bubble should be in the middle. I fared no better with this. The level could never be taken up and replaced with a certainty of the same indication; and reversing might go on for ever without arriving at a uniform or

consistent result. It became tiresome beyond endurance. The reason seemed to be—either that the ground flat surface of the level was not truly plane; or, invisible particles of dust settled on the speculum, and became attached to the level, or otherwise prevented its coming into true contact with the speculum. That the latter was not in fault was proved by a critical scrutiny of objects reflected in it with a telescope of considerable power.

I then ground some levels myself, and selected one having a scale of 5° of inclination, equal to a displacement of the bubble of two hundredths of an inch, which I mounted in a brass tube with three points or feet. This has proved permanently satisfactory, making the sextant thus complete as a travelling companion. After all, if a vessel of mercury is to be had, and no vibration forbids its use, I always employ it, preferring it to any levelled horizon whatever.

But what has induced me to venture to trouble the Society with all this detail, is that I have found much greater accuracy attainable with these small means than I had expected; and I think it possible that some Fellows of the Society, familiar with this kind of observation, may not be unwilling to hear what can be done with so small an instrument. It may be, however, that others have done as much before, without its having come under my notice; and I merely relate what I confess has excited my surprise.

I proceed now to extract a few observations from my journal.

1873, July 19.—Altitudes for time taken with three-inch sextant, power 11, mercurial horizon, Dollond roof. Times by sidereal watch (= S W) compared with sidereal clock (= S):—

	h	m	s		s
At	17	19	45	S W − S =	7·0
	19	43	35		5·0
	20	58	12		4·0

Arcturus. 1st mag. West. α Androm. 2nd mag. East.

Times.			Altitudes.			Results.	Times.			Altitudes.			Results.	
												Without roof.		
h	m	s	°	′	″	s	h	m	s	°	′	″		s
18	54	54·4	54	4	30	S. fast 0·5	20	28	22·8	88	41	0	S slow	1·5
	58	48·8	52	51	30	0·1		31	20·0	89	36	0		3·8
19	0	32·8	52	19	30	0·7		33	16·4	90	10	30	fast	1·4
	2	46·0	51	38	0	0·4		35	19·2	90	48	30	slow	1·7
	5	0·0	50	56	0	slow 0·3		37	21·6	91	25	45		1·3
	7	19·6	50	12	0	2·7		41	35·6	92	43	0		0·9
						Mean = S slow 0·22							Mean = S slow	1·81

Mean of both sets = S slow 1ˢ·01.

Index error accounted zero. Approximate position of Ray Lodge, latitude 51° 31′ 23″, longitude $2^m\ 49^s{\cdot}3$ west.

1873, July 28.—Same instrument, mercurial horizon and roof. Times by sidereal watch :—

	h	m	s		s
S =	17	59	43	S − S W =	43·0
	19	40	52		43·6
	21	13	52		44·4

α Coronæ. 2nd mag. West.

Times. h	m	s	Altitudes. °	′	″	Results. s
20	3	59·6	67	37	30 = S slow	22·0
	10	44·2	65	32	15	21·0
	13	36·8	64	39	0	20·1
	15	48·4	63	59	0	17·6
	17	54·0	63	19	30	19·7
	20	30·4	62	31	0	19·8
	22	20·4	61	57	0	19·8
					Mean = S slow	20·0

α Androm. 2nd mag. East.

Times. h	m	s	Altitudes. °	′	″	Results. s
20	40	33·2	92	46	0 = S slow	24·9
	43	8·0	93	31	15	19·2
	45	33·6	94	16	30	23·0
	49	59·6	95	36	30	22·2
	52	27·6	96	20	30	20·5
					Mean = S slow	21·96

Mean of both sets = S slow $20^s{\cdot}98$.

August 21.—Altitudes as before for time. Mercurial horizon without roof. Times by sidereal watch. S − S W = $1^m\ 59^s{\cdot}5$:—

α Androm. 2nd mag. East.

Times. h	m	s	Altitudes. °	′	″	Results. s
20	36	20·8	91	48	30 = S slow	12·9
	39	5·6	92	39	0	14·1
	41	20·8	93	20	0	13·9
	43	55·6	94	7	0	14·3
					Mean = S slow	13·80

α Coronæ. 2nd mag. West.

Times. h	m	s	Altitudes. °	′	″	Results. s
21	3	29·2	49	0	0 = S slow	13·6
	6	38·8	48	2	30	14·5
	8	54·8	47	22	0	13·5
	10	44·4	46	49	30	11·9
	13	21·2	46	1	30	15·1
					Mean = S slow	13·72

Mean of both sets = S slow $13^s{\cdot}76$.

Sept. 1.—Same instrument. Mercurial horizon without roof :—

	h	m	s		s
S =	20	54	34	S W − S =	42·0
	22	1	38		42·0
	23	52	52		43·6

Index error = + 20″

α Coronæ. 2nd mag. West.					Capella. 1st mag. East.				
Times. h m s	Altitudes. ° ′ ″		Results. s		Times. h m s	Altitudes. ° ′ ″		Results. s	
21 15 25·6	46 7 0	=S slow	32·7		22 44 57·6	62 37 30	=S slow	33·9	
19 48·8	44 48 0		33·1		51 51·6	64 22 30		34·2	
22 30·0	44 0 0		33·3		54 17·6	65 0 0		34·9	
24 36·4	43 22 15		33·5		56 45·6	65 38 0		34·9	
26 51·2	42 42 30		32·7		23 0 0·0	66 28 0		34·5	
	Mean = S slow		33·06			Mean = S slow		34·48	

Mean of both sets = S slow 33·77.

α Arietis. 2nd mag. East.					ζ Herculis. 3·2 mag. West.				
Times. h m s	Altitudes. ° ′ ″		Results. s		Times. h m s	Altitudes. ° ′ ″		Results. s	
22 18 45·6	78 8 30	=S slow	33·5		21 32 48·0	67 57 0	=S slow	28·3	
24 48·0	79 58 30		34·4		35 33·6	67 6 15		29·0	
27 44·0	80 50 0	windy	29·2		38 15·2	66 16 45		29·4	
					40 58·0	65 26 30		31·3	
					43 38·0	64 38 30		29·1	
	Mean = S slow		33·37			Mean = S slow		29·42	

Mean of both sets = S slow 31ˢ·40.

The last two sets of altitudes I should not depend much upon; and they are introduced principally to show the probable error under unfavourable circumstances, ζ Herculis not being bright enough, in the then state of the sky, and the altitudes of α Arietis being too few, and those disturbed by wind. Also a reason for rejecting α Arietis is the danger, without extreme care, of making α and β to osculate.

Sept. 22.—Mercurial horizon without roof. Sky hazy—increasingly so—so that at the close Capella was too dim to enable me to determine the index error. The index error, however, of this sextant is very small and very constant. Times by sidereal watch:

h m s		h m s	
S = 22 22 44	S W − S =	44·0	
23 56 47		45·4	

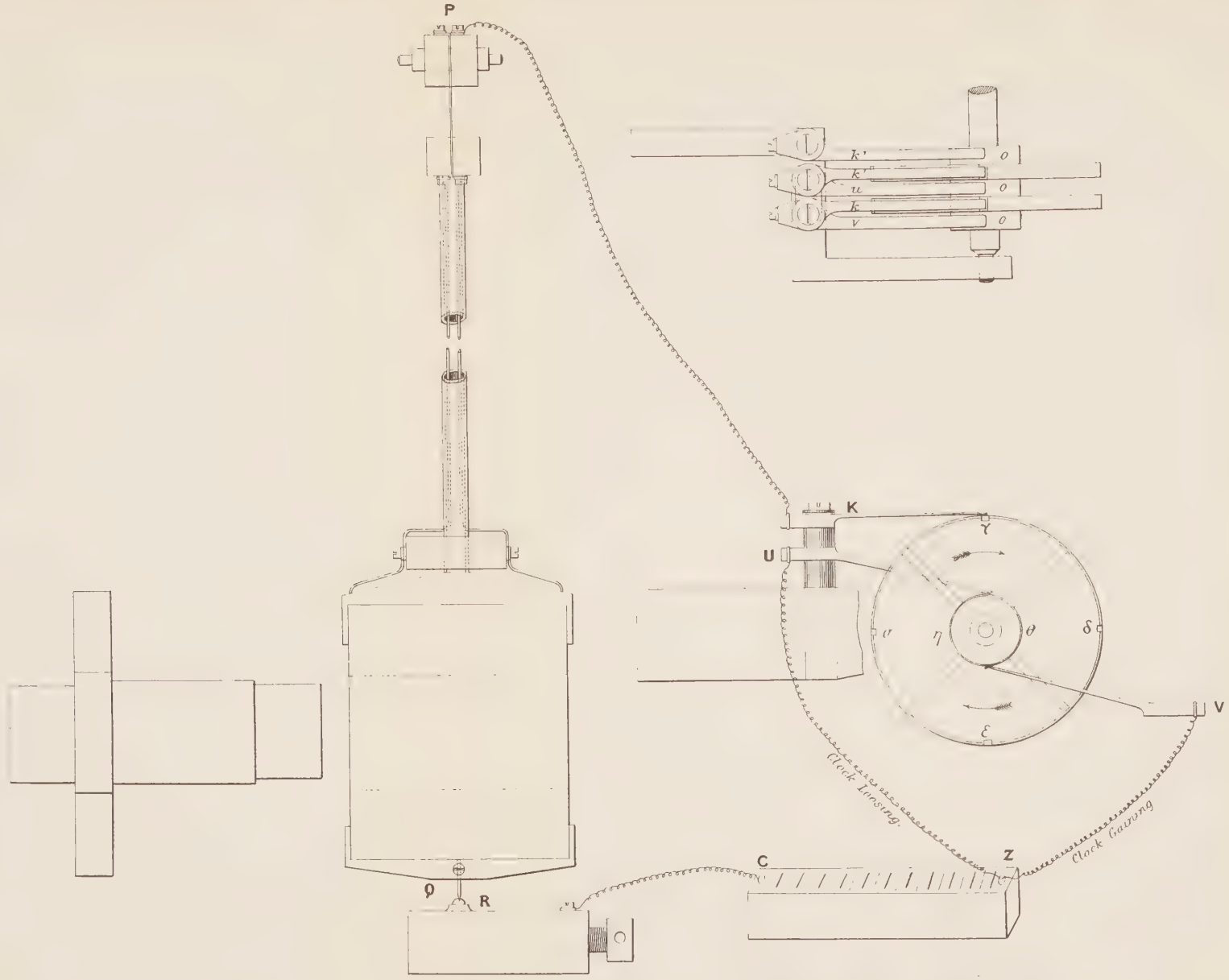
α Lyræ. 1st mag. West.					Capella. 1st mag. East.				
Times. h m s	Altitudes. ° ′ ″		Results. s		Times. h m s	Altitudes. ° ′ ″		Results. s	
22 46 50·0	89 57 0	=S slow	11·8		23 18 54·8	71 19 30	=S slow	12·6	
50 22 8	88 52 30		9·8		22 23·2	72 16 0		15·9	
54 10·4	87 41 45		13·6		26 52·0	73 27 0		11 8	
56 41·6	86 56 30		10·6		29 28·4	74 10 0		15·2	
59 36·0	86 2 0		14·7		33 1·2	75 7 30		14·7	
23 2 25·2	85 11 0		13·4		36 21·2	76 1 30		13·4	
	Mean = S slow		12·32			Mean = S slow		13·93	

Mean of both sets = S slow 13ˢ·12.

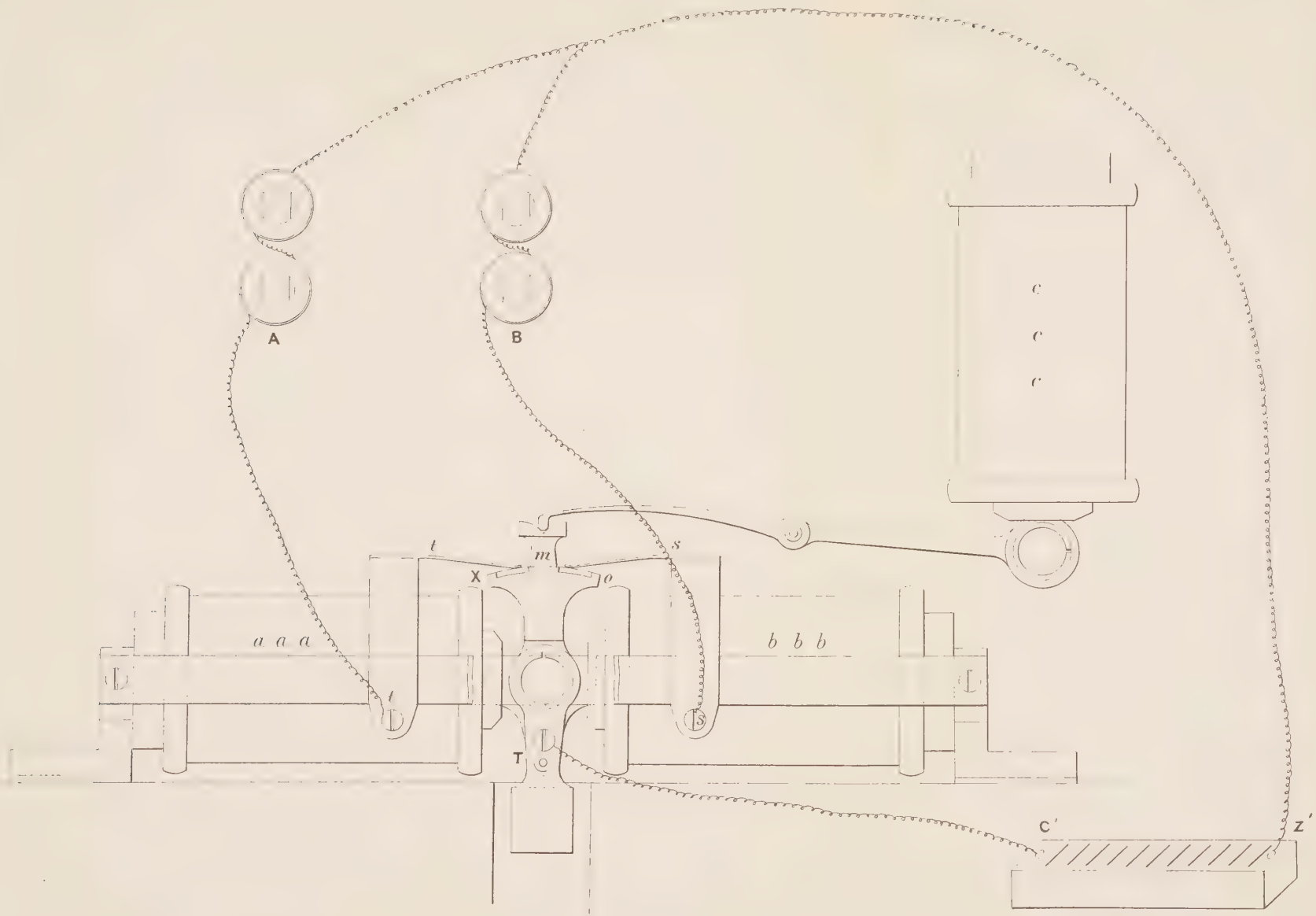
The discrepancies of the individual altitudes will not seem extravagant when it is remembered that Capella, for instance, while these were taken, was rising about 16″ of altitude in one second of time, the sextant only reading to 30″; and I may add to this my deteriorated vision, by which I am often sorely puzzled to read off the sextant in any degree to my satisfaction. I think it unnecessary to add that there are *no altitudes rejected*, the whole series being given; and that the whole of the observations are, I believe, as far from being 'cooked' as is possible. It is my practice to work out each altitude separately; as, if grouped together and the mean of the altitudes and times taken, a blunder in one observation would be masked and might vitiate the whole. The altitudes of this evening, thus treated, give for α Lyræ S. slow $12^s{\cdot}0$, and for Capella $15^s{\cdot}3$ mean $= 13^s{\cdot}6$, agreeing within about half a second with the mean of the individual results. When there is a perceptible breeze I do not hesitate to use the Dollond roof, generally taking half the number of altitudes with the roof in a given position, and reversing it for the other half. I do not find, however, any sensible error traceable to the roof. Altitudes of the Sun I very rarely take; for, although it is a more agreeable and easier operation, and the sextant is more accurately read by sunlight than lamplight, there is the disadvantage of not having any other heavenly body whose altitude can be taken at about the same time, to balance or correct the solar observation.

For keeping and carrying on my time, when I have got it, I generally use a mural transit, being a telescope firmly braced to a thick wall, and directed approximately to the equator, and within a few seconds of the meridian; its exact deviation from which having been originally determined and occasionally verified by sextant observations. The object-glass is an excellent one of 2·20 inches aperture and 25·8 inches focus, the eye-piece magnifying 32·6 times. It has three rather stout wires, about 40 seconds apart, and is furnished with the means of illumination. The field of view is large, and, by means of a positive eye-piece sliding vertically, it embraces all stars between about 88° 15′ and 91° 45′ polar distance. My observing list includes about 170 stars, not lower than the seventh magnitude of the British Association Catalogue. Of this magnitude, however, only about 27 are introduced in parts of the sky rather bare of larger stars, and these are abundantly bright even in a somewhat hazy and dull sky.

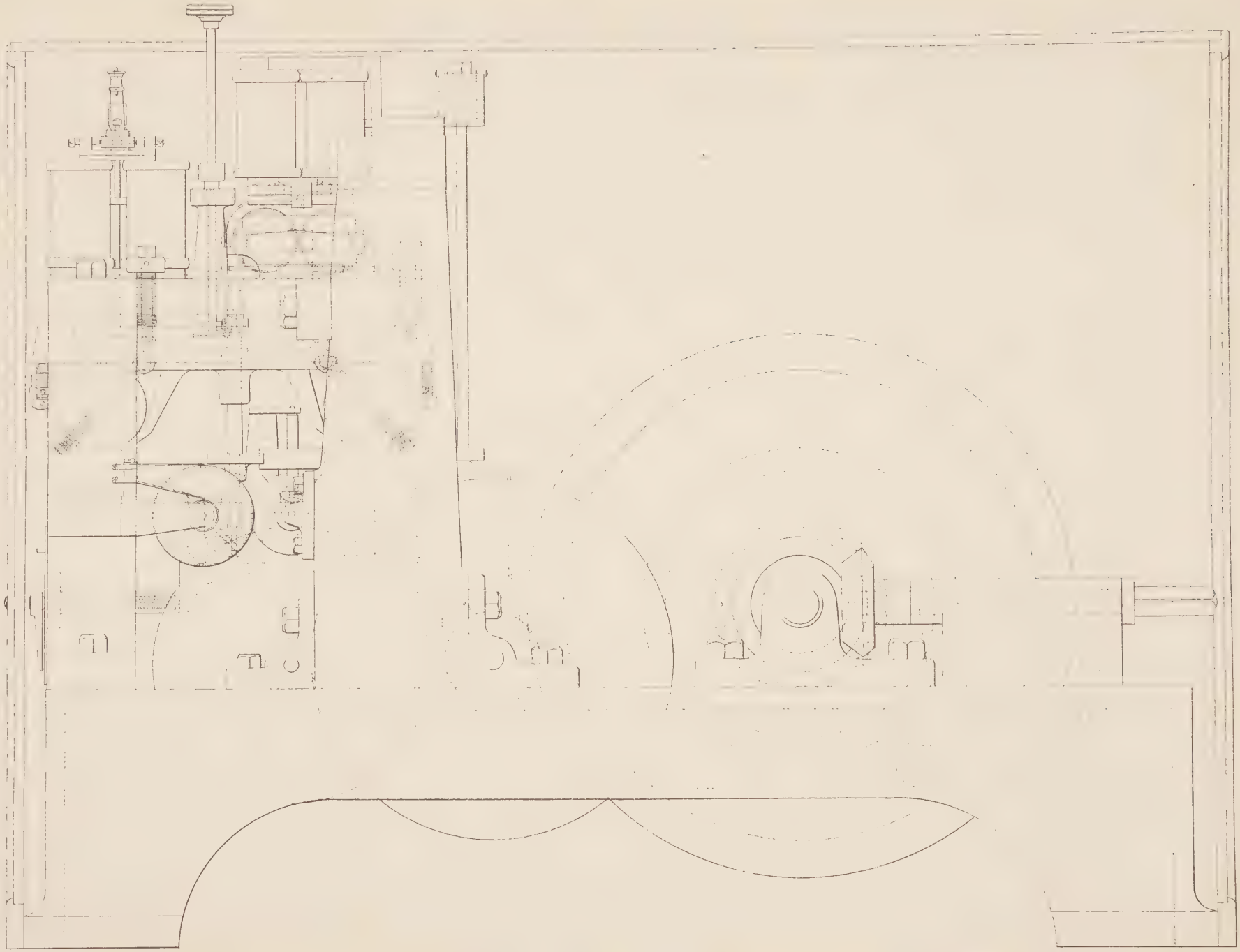
The annual precessions are stated in my transit-list, and the corrections of R.A. due to the season of the year are obtained by inspection from a short supplemental table. Of course I do not pretend thus to ensure the extremest accuracy, my requirements generally being satisfied with the belief or conviction that the probable error of any determination does not exceed one second. The large number of stars passing ensures, even on a fitful night, frequent opportunities of observation.



Malby & Sons, Lith.



Malby & Sons, Lith.



I have made with the sextant above described equally coincident observations for determination of latitude, but with these I do not think it necessary to trouble the Society.

Ray Lodge, Maidenhead, 1873, *November* 10.

On a New Driving Clock for Equatorials. By Lord Lindsay and David Gill, jun.

One of the most important adjuncts of the modern equatorial is the driving clock. Formerly the equatorial was merely expected to carry a small and light micrometer, and that with only moderate accuracy for a short time.

Now spectrum analysis and celestial photography demand a higher accuracy of rate, and that maintained for a considerable time.

Not only is it so, but this accuracy has to be attained under more unfavourable circumstances. Modern spectroscopes are of such considerable weight, and so unsymmetrical in shape, that it is difficult or impossible to maintain in all circumstances a perfect counterpoise of the equatorial.

From these considerations we endeavoured to realise the following conditions, in contriving a clock-motion for the great equatorial of the Dunecht Observatory.

1. A driving power four or five times as great as that actually required, so as to avoid failure when the balancing of the instrument is defective.
2. A rate of motion equal in accuracy to that of a good sidereal clock.
3. Maintenance of the same uniformity of rate under change of work to be done, twice as great as that to which the clock will be likely to be submitted.
4. An easy and rapid means of adjusting that rate and so avoiding delay in timing as much as possible.

The first of these conditions was easily attained by the employment of a large and powerful clock capable of being driven by a very heavy weight as in the accompanying sketch. The last three conditions are at once fulfilled by controlling a rotatory governor by a swinging pendulum, the latter being controlled electrically by the standard clock of the Observatory.

The Bonds in America, and the late Mr. Cooke of York, have both contrived clocks in which the rotation of a fan is controlled by a swinging pendulum, but in neither of these arrangements is the control perfect. In either construction a moderate increase of the driving weight will cause the clock to *run up* a certain amount before the control of the swinging pendulum is sufficient to overcome the increased tendency to gain. The effect of such

a property is that under variation of resistance or power, the image of a star, though retained *near* the web during long intervals, will be found to oscillate from one side of the web to the other, a property fatal in photography, and troublesome in all other kinds of observation, except perhaps with a Heliometer, where such oscillations would be of no consequence, and the good general rate of the clock an advantage. After many experiments, which we shall not detail here, we have contrived the following arrangement, and the results of its adoption are so satisfactory that we are induced to think its description may be acceptable to the Society.

In fig. 1, *a k n* and *b l m* are steel bars (tipped with leather at the lower extremity) and sliding freely in the holes *k l m n*.

They would continually rub on the ring C D, which is attached to the axis of the governor balls, were they not sometimes kept from doing so by the arms *e f* and *g h*.

These arms are attached to the armatures of the electro-magnets A and B, and are pivotted at the points *p* and *q* respectively.

From inspection of the drawing, it is evident that in the normal condition, when no current is passed through A or B, the arm *e f* (carrying with it the bar *a k n*) will be raised by the action of the spiral spring α, whilst the bar *b l m* will be allowed to rest on the ring C D, its support, the arm *g h* being removed by the spiral spring β.

This is the normal condition of the clock, and the governor is rated so as to keep very nearly true time when the bar *a k n* is raised from the ring C D, and when *b l m* rests upon it.

It is evident now, that we can retard the clock by lowering the bar *a k n*, so as to rest on the ring C D, or we can accelerate the clock by raising the bar *b l m* from its normal pressure on C D.

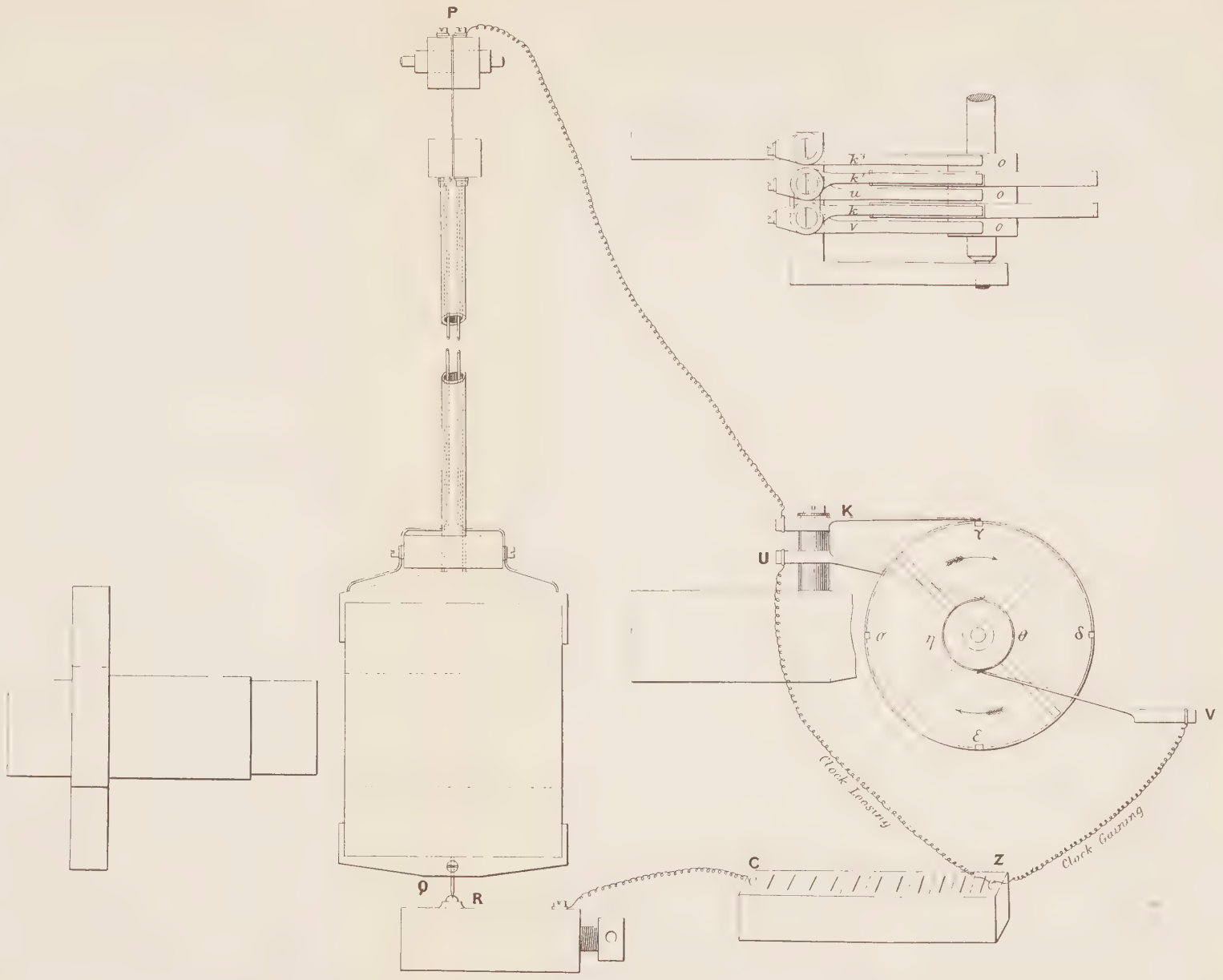
That is to say, to retard the clock-rate we must excite the electro-magnet A, and to accelerate it excite the electro-magnet B.

To send these currents of control, a Bain's Pendulum (fig. 2), P Q, is *driven* by currents from contact springs applied to the standard clock in the usual way.

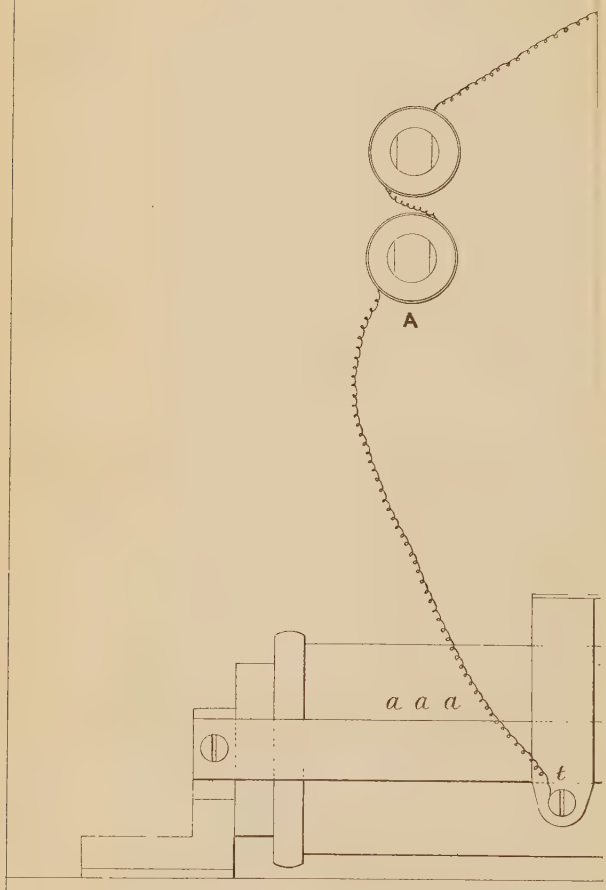
A platinum point attached to the lower extremity of the pendulum at Q cuts through a globule of mercury R. As the arc of vibration of the pendulum is very large (viz. 7°), the contact with the mercury being very slight, and made at the lowest point of vibration, where the motion is most rapid, it may practically be considered instantaneous. This platinum point is in electrical connection with a terminal at P.

Upon a spindle of the clock, which revolves once a second, is fixed a vulcanite disc $\gamma\ \delta\ \varepsilon\ \sigma$. This disc is surrounded by a ring of silver, except at the four points $\gamma\ \delta\ \varepsilon\ \sigma$, where pieces of ivory are inserted and turned flush with the silver.

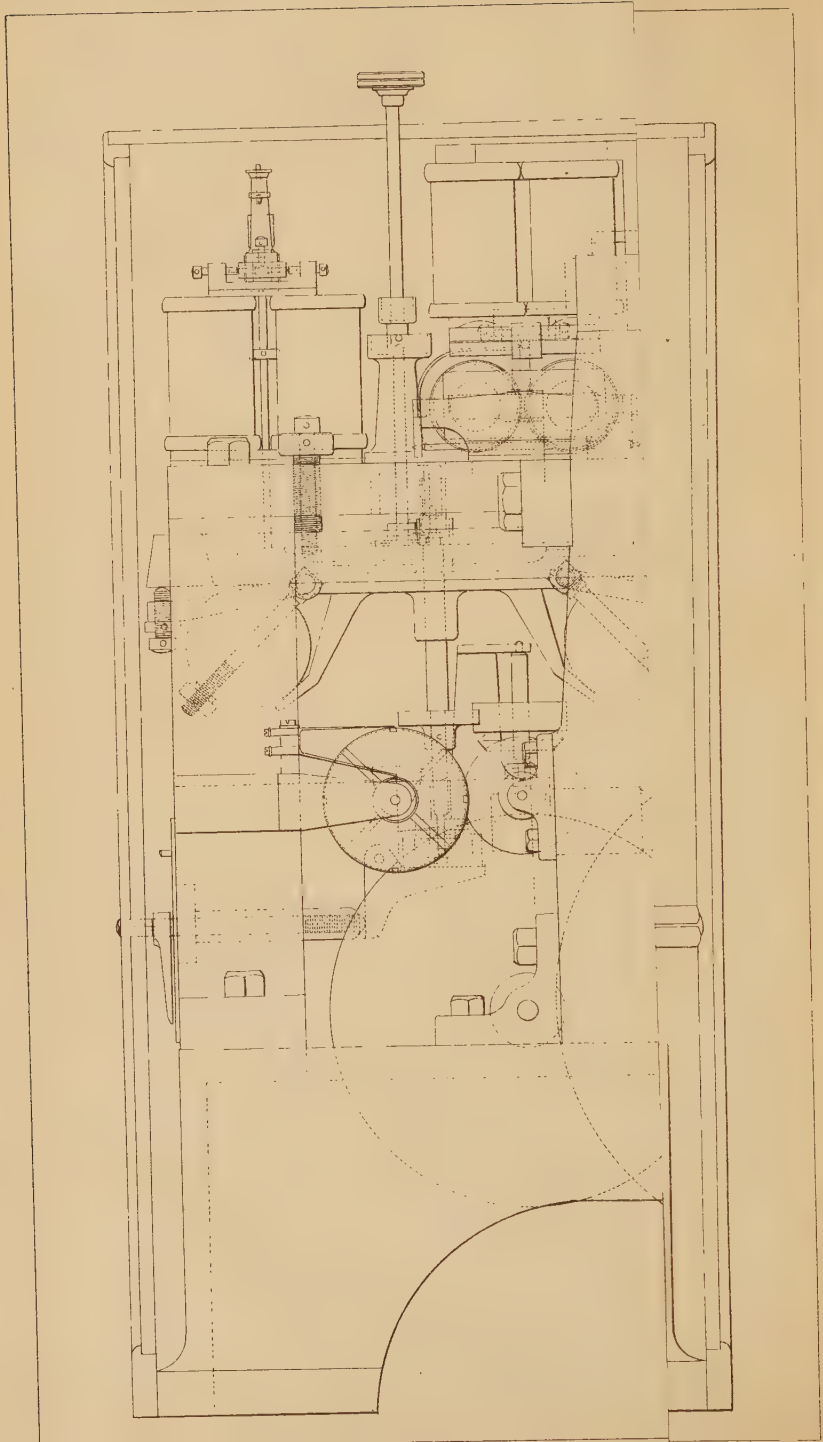
Upon the same axis, and on both sides of the disc $\gamma\ \delta\ \varepsilon\ \sigma$ are two smaller discs surrounded completely by silver. These



Malby & Sons, Lith.



Sons, Lith



discs are marked $\eta\ \theta$ in fig. 2, that on further side of $\gamma\ \delta\ \epsilon\ \sigma$, and invisible in the drawing, we shall call $\eta'\ \theta'$.

The segments of the large silver ring $\sigma\ \gamma$ and $\delta\ \epsilon$ are connected by a slip of silver with $\eta\ \theta$, and $\gamma\ \delta$ and $\epsilon\ \sigma$ are connected to the other disc $\eta'\ \theta'$ on the other side of $\gamma\ \delta\ \epsilon\ \sigma$.

Upon the large disc presses the spring K, and on the smaller discs $\eta\ \theta$, and $\eta'\ \theta'$ the springs V and U respectively.

Then if the spring K rests on the point γ it is insulated, if on $\sigma\ \gamma$ or $\epsilon\ \delta$ it is in electric connection with the spring V, if on $\gamma\ \delta$ or $\epsilon\ \sigma$ it will be in connection with the spring U.

If now the spring K is connected to the terminal P, the mercurial reservoir of globule R to one terminal C of a battery (Plate II.), and the springs U and V to the other terminal, Z, of the same battery. If also the vulcanite disc revolves in the direction of the arrow heads, we see that when the instantaneous contact of the platinum point Q with the mercury at R occurs, at the instant that the spring K presses on the insulated point γ, no current whatever can pass—that is to say, no current can pass when the rotatory governor and swinging pendulum are in perfect sympathy.

If, however, during the next vibration of the pendulum the governor gains upon the swinging pendulum, then at the instant of next instantaneous contact the spring K will not rest on γ, but on some part of the segment $\gamma\ \sigma$ near γ, and a current will pass from C through R Q P K $\eta\ \theta$ V to Z, and similarly if the governor lose, K will then be on $\gamma\ \delta$ and the current will pass through $\eta'\ \theta'$ and U to Z.

That is, for *clock gaining* the instantaneous control current will flash through V Z, and for *clock losing* through U Z.

But *directly* these instantaneous currents would be useless for the purposes of control; accordingly a switch of the following description is employed, by which the accelerating or retarding effects are made to last during the entire second.

Plate III. *a a a* and *b b b* are electro-magnets, the helix of *a a a* being placed in the circuit of V Z, and that of *b b b* in the circuit of U Z.

The switch X *m o* T is pivotted at T.

Its surface X *m o*, on which the springs *t* and *s* rest, is insulated by ivory, except at *m*.

A second battery, C′ Z′, has one terminal connected to this switch, the other from Z′, through A to *t* and from Z′ through B to S.

If now the governor is fast of the pendulum we shall first have an instantaneous current through V Z, which excites *a a a*, attracts the switch X *m o*, which turns on a current to excite A, which lowers the arm *e f* (Plate I.), which lowers the bar *a k n* upon the ring C D, which retards the clock; and *vice versâ*.

As now described the switch would always either accelerate or retard, but there is a simple arrangement by which it is drawn into the centre, or neutral point, when there is no need for correction.

This is simply another disc on the same spindle, the reverse of the disc $\gamma\ \delta\ \varepsilon\ \sigma$—that is to say, entirely insulated on the edge *except* at the points γ and ε, which we shall call $\gamma'\ \varepsilon'$.

There is a spring like K, which we shall call K′, pressing on this disc, and when the instantaneous current takes place, when K′ is at γ' it is made to pass through an electro-magnet $c\ c\ c$, and from the drawing (Plate III.), the manner in which the switch is brought to the centre will be evident.

The whole apparatus may appear somewhat complicated from this description, though, indeed, it is not so.

The power of control depends on the weight of the bars $a\ k\ n$ and $b\ l\ m$, and the character of the friction surfaces, and can be almost indefinitely regulated, though, of course, when pushed too far, at the expense of uniformity of motion. It remains to be considered how far uniformity of motion is sacrificed by this control. All control must consist of oscillation, and all we can do is to reduce that oscillation to a harmless amount.

We have found from experiment that *without control* no change of work which the clock can be called upon to perform, that is, from driving only itself to driving the telescope, and that considerably and disadvantageously out of balance, affects the clock rate $\frac{1}{100}$ part, therefore the clock can commit no error in one second so great as the $\frac{1}{100}$ second.

We have adjusted the extreme limits of control to correspond to one second per minute; that is, that the clock may err $\frac{1}{120}$ second on either side of the point of sympathy in a second, and would be corrected during the succeeding second.

The effect is to produce a control so perfect that *no variation whatever* can be perceived on the second hand of the driving clock from that of the standard clock, during any number of hours, and no sensible oscillation of a star relative to the micrometer web under high powers.

It is no exaggeration to say that when the clock is driving the telescope the observer may add his own weight to the driving weight without affecting the accuracy of control.

The construction of the disc $\gamma\ \delta\ \varepsilon\ \sigma$, we have just described, permits the use of instantaneous currents at half-second intervals, and thus the power of control can be doubled, or the oscillation halved at pleasure. We have found the currents from a seconds pendulum however so entirely satisfactory that we have not as yet employed a half seconds pendulum.

The clock is the workmanship of Messrs. T. Cooke and Sons, York, and is most highly creditable to their firm.

We must conclude by expressing our obligations to them for the trouble they have taken, and the intelligent manner in which they have carried out our designs.

The Observatory, Dunecht.

Note on the Star 515 *in Oeltzen's Catalogue of Schwerd's Stars.*
By R. C. Carrington, F.R.S.

It will be recollected that at page 150 of my Catalogue of Circumpolar Stars, I added this note:—

"There is one star in Oeltzen's Catalogue, No. 515, of the 7th magnitude, observed once only by Schwerd, the position of which for 1855·0 is

R.A. = $8^h\ 21^m\ 43^s{\cdot}1$. N.P.D. = $6°\ 46'\ 58''{\cdot}1$.

which I have no observation of, and do not find to exist. Mr. Oeltzen finds no error in his reduction on re-examination."

I have frequently referred to this place in the heavens, and have found no trace of the star, and it became an object with me to find what error Schwerd had made in his Catalogue. The place stands thus in the Catalogue:

1826, Oct. 19.

No.		Mag.	1	2	3	8	4	9	5	6	7
							h m s	m s	s		
753	S.P.	7	—	—	—	—	20 15 43:	16 32·0	24·5	—	—
			Nonien.		I.	III.	II. IV.	Niveau.	Fad.	Decln.	
			222° 38′ 5″			5″	— —	−10″·9+10″·2	9	83° 18′	

Apparently all correct. But it must be noted that 83° 18′ is not observed, but deduced from 222° 38′; and $20^h\ 15^m\ 43^s$: is the mean wire deduced also from 83° 18′, on the supposition that the minute of wire 5 (3 really) is 18^m. Now it struck me to try 17^m as the minute, and to see what change that would make in the reduction, and I found it would require a change of 8° in the declination. Accordingly I supposed 214° 38·5″ | 5″ and 75° 18′ to be the declination of the star, and then found

h	m	s		
8	16	7·17	for mean wire	
8	16	24·60	for App. R.A.	
8	16	32·71	for R.A. 1828·0	= $8^h\ 19^m\ 49^s{\cdot}88$ for 1855·0
°	′	″		
75	19	2″·2	observed decln.	
—	1	24·2	refraction	
75	17	38·0	for App. Dec.	
75	17	47·7	for Dec. 1828·0	= 75° 12′ 40″·7 for 1855·0

which shows that the star 515 of Oeltzen's Catalogue is a previous observation, under disguise, of his next star 516.

They are both found in Argelander's Zone, 75°, under No. 342, of 6·4 magnitude and $8^h\ 19^m\ 48^s - 75°\ 13'{\cdot}2$ — for 1855·0.

There can be no doubt that 214° should be the circle reading, and that Schwerd did not put it down at the time.

Observations of the Periodical Comets of Tempel and Brorsen. By George Bishop, Esq.

I beg to communicate to the Society the following positions of the periodical comet of *Tempel* (discovered July 3) and of *Brorsen's* comet of short period, obtained in this observatory. The observations have been chiefly made by Mr. Plummer. Three by Mr. Hind are distinguished by H.

Tempel's Comet.

	Twickenham Mean Time. h m s	Apparent R.A. h m s	Apparent Decl. ° ′ ″	Observer.
July 31	13 24 33	1 18 44·46	−7 54 4·3	P
Aug. 2	13 2 58	1 22 31 59	8 15 17·1	H
„ 6	14 21 22	1 29 36·92	9 3 11·6	H
Sept. 17	13 52 22	1 49 47·13	−18 21 11·5	P
„ 20	13 51 57	* — 53·14	*— 3 28·3	„
„ 21	14 5 0	* — 46·46	*—10 25·8	„
„ 26	13 41 7	1 43 13·29	−19 37 20·5	„
Oct. 20	11 35 17	1 22 10·1	19 55 41	„
„ 20	11 39 49	1 22 11·4	−19 55 59	H

The stars with which the comet was compared on the nights of the 20th and 21st September are not found in any catalogue, and on the 20th October, owing to the extreme faintness of the comet, the observers placed no reliance on their observations. For this night the position is given approximately.

Brorsen's Comet.

	Twickenham Mean Time. h m s	Apparent R.A. h m s	Apparent Decl. ° ′ ″
Sept. 17	15 45 39	8 50 7·44	+ 3 23 45·6
„ 21	16 8 5	9 17 24·24	4 36 40·1
Oct. 8	16 32 26	11 13 2·66	7 37 17·7
„ 15	18 9 25	11 59 19·11	7 21 0·4
„ 26	17 16 18	13 2 22·09	+ 5 30 26·8

Oct. 8. The comet observed through fog.

„ 15. The observation rendered doubtful owing to fog and the near approach of daylight.

Oct. 26. The comet's diameter estimated at $1\frac{1}{4}$ minutes. The comet round, with considerable condensation towards the centre.

The Observatory, Twickenham,
1873, *Nov.* 12.

Parabolic Elements of the Comets of Henry (Paris) and Borrelly (Marseilles). By Mr. W. E. Plummer.

(*Communicated by George Bishop, Esq.*)

The following elements of Comet IV. 1873, discovered at Paris by Paul Henry on August 23, are computed from obser-

vations made at Washington on August 25 and September 2, and at Twickenham on September 12. Corrections for parallax and aberration have been applied, computed from the orbit of Mr. Ormond Stone.

Perihelion Passage. Oct. 1·76295 G.M.T.

		o ′ ″	
π	...	302 58 51·0	M. Eq. Sept. 0.
Ω	...	176 43 35·4	
i	...	58 30 40·1	
Log. q	...	9·5852771	

Motion Retrograde.

A comparison with the middle place shows the following errors in the sense, calculation-observation.

Longitude $-1''.8$. Latitude $-1''.6$.

The orbit of Comet III. 1873, discovered by M. Borrelly, is computed from observations made at Marseilles on August 23, at Königsberg on September 2, and at Twickenham on September 17, corrected for parallax and aberration deduced from elements computed by myself, but embracing a shorter interval, and to which the following are preferable.

Perihelion Passage. Sept. 10·73619 G.M.T.

		o ′ ″	
π	...	36 57 3·7	M. Eq. Sept. 0.
Ω	...	230 38 37·2	
i	...	84 3 20·1	
Log. q	...	9·9002537	

Motion Retrograde.

The errors for the middle observation are—

C−O $\delta\lambda \cos\beta$ $-15''.8$

$\delta\beta$ $-6''.2$

Mr. Bishop's Observatory, Twickenham,
1873, *November* 12.

Observations of Comets and Minor Planets made at the Observatory of Marseilles.

(*Communicated by M. Stéphan through Mr. Hind.*)

Comet II. 1873 (*Tempel*).

1873	Mean Time Marseilles. h m s	R.A. h m s	Log. fac. par.	N.P.D. ° ′ ″	Log. fac. par.	Comp. Star.	Obs.
July 29	13 57 39	1 14 46·05	−9·463	97 32 19·1	−0·8300	*a*	B
30	14 13 32	1 16 48·28	−9·422	97 43 9·5	−0·8293	*a*	S
31	15 3 32	1 18 50·66	−9·252	97 54 22·4	−0·8369	*b*	C
Aug. 30	13 55 31	1 52 55·99	−9·187	104 32 57·9	−0·8721	*c*	B
Sept. 18	12 28 51	1 49 13·36	−9·237	108 30 38·3	−0·8869	*d*	B
19	11 42 53	1 48 36·01	−9 387	108 40 1·2	−0·8776	*d*	B
24	13 44 51	1 44 54·49	+8·498	109 23 48·6	−0·8987	*e*	C

Comet III. 1873 (*Borrelly*).

Aug. 20	15 43 40	7 27 1·57	−9·744	51 15 46·5	−0·6509	*f*	B
21	14 54 14	7 28 20·02	−9·736	52 13 26·5	−0·6299	*g*	S
26	15 41 49	7 35 33·60	−9·708	57 45 29·2	−0·6814	*h*	B
30	14 58 43	7 41 56·44	−9·689	62 51 19·6	−0·7287	*i*	B
Sept. 1	14 40 51	7 45 5·65	−9·677	65 41 5·7	−0·7277	*k*	B
16	16 23 32	8 21 31·77	−9·594	93 48 32·2	−0·7976	*l*	B
18	16 29 45	8 27 51·59	−9·586	98 16 16·0	−0·8118	*m*	B

Comet IV. 1873 (*Henry*).

Aug. 24	12 48 24	7 33 24·77	−9·925	30 44 51·9	−0·3712	*n*	S

Comet V. 1873 (*Brorsen*).

Sept. 20	15 19 2	9 10 26·31	....	85 40 49·2	...	*o*	S
24	16 59 6	9 38 1·99	...	84 32 51·0	...	*p*	C

Comet VI. 1873 (*Faye*).

Sept. 3	16 9 3	7 0 48·14	...	74 12 35·2	...	*q*	S

The observations of Comets V. and VI. are corrected for the effect of parallax.

Mean Positions of the Comparison-Stars for 1873·0.

		Name of Star.	Mag.	Mean R.A. h m s	Mean N.P.D. ° ′ ″	Authority.
Comet II. 1873	*a*	W. B. (1) I. 212	9	1 14 37·70	97 37 27·6	Weisse's / Bessel.
„	*b*	W. B. (1) I. 271	7	1 17 57·38	97 34 42·7	„
„	*c*	W. B. (1) I. 930	7	1 53 21·56	104 29 27·1	„
„	*d*	Lalande 3631	8	1 51 11·75	108 17 39·0	Lalande.
„	*e*	Lalande 3339	9	1 42 45·89	109 0 53·9	„
Comet III. 1873	*f*	W. B. (2) VII. 887	6	7 31 41·82	51 22 1·6	Weis. Bes.
„	*g*	W. B. (2) VII. 684	8	7 24 55·79	52 24 14·6	„
„	*h*	W. B. (2) VII. 996	9	7 34 48·68	57 41 26·6	„
„	*i*	B. A. C. 2617	5	7 45 43·46	62 54 25·9	B. A. C.
„	*k*	W. B. (2) VII. 1206	7	7 43 1·16	65 31 17·2	Weis. Bes.
„	*l*	B. A. C. 2831	6	8 20 6·41	93 34 16·2	B. A. C.
„	*m*	W. B. (1) VIII. 804	7	8 31 39·49	98 26 16·7	Weis. Bes.
Comet IV. 1873	*n*	Oeltz. Arg. 8087	8–9	7 29 37·07	30 37 0·4	Oeltz. Arg.
Comet V. 1873	*o*	W. B. (1) IX. 102	8	9 6 46·52	85 39 15·7	Weis. Bes.
„	*p*	Lalande 19072	9	9 36 13·42	84 23 44·0	Lalande.
Comet VI. 1873	*q*	W. B. (2) VII. 11	9	7 3 4·46	74 11 18·2	Weis. Bes.

Observations of Minor Planets (133) *and Sophrosyne* (134).

(133) *Watson.*

	Mean Time Marseilles. h m s	R.A. h m s	Log. fac. par.	N.P.D. ° ′ ″	Log. fac. par.	Comp. Star.	Obs.
Aug. 19	14 10 39	23 0 38·20	+9·079	92 43 30·6	−0·8059	*a*	S
20	12 36 48	22 59 56·30	−8·682	92 45 30·7	−0·8069	*a*	S
21	13 29 15	22 59 6·67	+8·775	92 47 41·7	−0·8073	*b*	S
28	9 46 2	22 53 45·25	−9·445	93 5 0·7	−0·8032	*c*	B
29	8 44 34	22 52 58·83	−9·547	93 7 34·1	−0·7995	*c*	B
30	9 13 55	22 52 9·12	−9·491	93 10 28·5	−0·8015	*c*	B
Sept. 1	9 14 31	22 50 31·17	−9·472	93 16 11·3	−0·8039	*c*	B
19	9 11 46	22 36 35·84	−9·220	94 9 32·2	−0·8147	*d*	B
24	9 36 26	22 33 19·80	−8·898	94 23 58·4	−0·8182	*e*	C
25	9 56 6	22 32 42·64	−8·619	94 26 45·4	−0·8190	*e*	C
27	9 46 4	22 31 33·30	−8·536	94 31 32·9	−0·8194	*e*	C
28	9 56 8	22 30 59·36	...	94 33 54·1	−0·8199	*e*	C
29	10 7 10	22 30 26·73	+8·358	94 36 18·0	−0·8201	*e*	C

Sophrosyne (134) (*Luther*).

	Mean Time Marseilles.	R.A.	Log. fac. par.	N.P.D.	Log. fac. par.	Comp. Star.	Obs.
	h m s	h m s		° ′ ″			
Oct. 4	11 49 42	0 1 1·77	+8·918	82 14 29·1	−0·7147	*f*	S
10	10 54 30	23 55 13·62	+8·524	82 23 1·7	−0·7152	*f*	S
12	11 32 27	23 53 22·65	+9·091	82 26 1·4	−0·7186	*f*	S

Mean Positions of the Comparison-Stars for 1873·0.

		Name of Star.	Mag.	Mean R.A.	Mean N.P.D.	Authority.
				h m s	° ″	
(133)	*a*	W. B. (1) XXII. 1237	9	22 59 26·99	92 46 22·7	Weis. Bes.
,,	*b*	W. B. (1) XXIII. 3	8–9	23 2 31·10	92 56 36·7	,,
,,	*c*	W. B. (1) XXII. 1090	9	22 53 16·67	93 7 15·8	,,
,,	*d*	W. B. (1) XXII. 731	8–9	22 35 32·03	94 8 11·8	,,
,,	*e*	Lalande 46228	9	22 31 45·20	94 22 52·9	Lalande.
Sophrosyne	*f*	B. A. C. 8354	6	23 56 0·50	82 13 9·6	B. A. C.

Comparison of the R.A. and N.P.D. of Standard Stars observed at the Radcliffe Observatory, Oxford, in the year 1870, *with the R.A. and N.P.D. founded on the Tabulæ Reductionum.* By Prof. Dr. F. Ph. Wolfers.

(*Communicated by the Radcliffe Observer.*)

Name of Star.	No. of Obs.	R. A. 1870. Oxford.	R. A. 1870. W.	O—W.	No. of Obs.	Decl. 1870. Oxford.	Decl. 1870. W.	O—W.
		h m s	°	s		° ′ ″	″	″
α Androm.	13	0 1 40·28	40·30	−0·02	17	+28 22 22·47	22·43	+0·04
γ Pegasi	1	0 6 32·68	32·69	−0·01	5	+14 27 39·25	38·91	+0·34
α Cassiop.	8	0 33 8·53	8·65	−0·12	15	+55 49 25·56	26·61	−1·05
α Arietis	4	1 59 50·99	50·99	0·00	5	+22 50 47·13	47·64	−0·51
γ Ceti	2	2 36 33·97	34·04	−0·07	1	+ 2 41 11·31	11·03	+0·28
α Ceti	1	2 55 29·06	29·15	−0·09	1	+ 3 34 43·83	40·66	+3·17
[δ Arietis]	2	3 4 11·94	12·02	−0·08	2	+19 13 59·86	59·43	+0·43
α Persei	9	3 15 3 13	3·27	−0·14	18	+49 23 45·05	45·16	−0·11
α Tauri	11	4 28 27·73	27·82	−0·09	18	+16 14 44·03	44·91	−0·88
α Aurigæ	8	5 7 5·25	5·40	−0·15	12	+45 51 43·81	45·40	−1.59

Name of Star.	No. of Obs.	R. A. 1870. Oxford. h	m	s	W. °	O—W. s	No. of Obs.	Decl. 1870. Oxford. °	′	″	W. ″	O—W. ″
β Orionis	7	5	8	17·42	17·52	−0·10	9	− 8	21	14·86	14·59	−0·27
β Tauri	9	5	18	4·48	4·56	−0·08	11	+28	29	41·16	40·73	+0·43
α Orionis	9	5	48	8·01	8·12	−0·11	10	+ 7	22	48·24	48·85	−0·61
α Canis Maj.	25	6	39	25·02	25·05	−0·03	26	−16	32	23·68	23·81	+0·13
α Canis Min.	19	7	32	29·67	29·75	−0·08	20	+ 5	33	20·90	21·36	−0·46
β Geminor.	13	7	37	21·49	21·52	−0·03	16	+28	20	16·31	15·90	+0·41
ι Urs. Maj.	...		...		...	...	2	+48	33	0·69	59·35	+1·34
α Hydræ	3	9	21	11·87	11·98	−0·11	2	− 8	5	46·72	46·65	−0·07
θ Urs. Maj.	...		...		...	...	5	+52	16	4·61	4·90	−0·29
α Leonis	4	10	1	26·73	26·82	−0·09	6	+12	36	5·82	5·84	−0·02
[γ¹ Leonis]	6	10	12	48·12	48·14	−0·02	6	+20	29	53·17	53·46	−0·29
[χ Leonis]	2	10	58	18·64	18·71	−0·07	3	+ 8	2	17·76	17·00	+0·76
[δ Leonis]	7	11	7	11·51	11·64	−0·13	12	+21	14	9·13	8·51	+0·62
[δ Crateris]	6	11	12	50·60	50·64	−0·04	5	−14	4	30·76	31·65	+0·89
β Leonis	1	11	42	25·59	25·67	−0·08	3	+15	17	55·83	55·95	−0·12
β Virginis	1	11	43	55·26	55·41	−0·15	1	+ 2	29	50·39	50·15	+0·24
γ Urs. Maj.	1	11	46	58·83	59·01	−0·18	17	+54	25	3·18	3·03	+0·15
[γ¹ Virginis]	2	12	35	4·29	4·53	−0·24	2	− 0	44	6·34	9·19	+2·85
[12 Canum Venat.]	...		...		...	...	4	+39	1	16·83	15·84	+0·99
α Virginis	...		...		...	...	2	−10	28	52·92	54·65	+1·73
[ζ Virginis]	3	13	28	4·27	4·38	−0·11	4	+ 0	4	9·25	12·77	−3·52
η Urs. Maj.	3	13	42	25·03	24·99	+0·04	9	+49	57	46·29	46·73	−0·44
[η Bootis]	7	13	48	29·62	29·82	−0·20	15	+19	3	2·10	2·44	−0·34
α Bootis	6	14	9	43·92	43·96	−0·04	11	+19	51	37·85	38·47	−0·62
α Libræ	5	14	43	41·41	41·41	0·00	9	−15	29	57·98	58·67	+0·69
β Urs. Min.	11	14	51	6·38	6·66	−0·28	32	+74	41	12·56	10·69	+1·87
[ψ Bootis]	2	14	58	52·61	52·58	+0·03	5	+27	27	22·78	22·70	+0·08
α Coronæ	6	15	29	11·07	11·11	−0·04	25	+27	9	14.01	14·83	−0·82
α Serpentis	6	15	37	51·93	51·99	−0·06	7	+ 6	50	10·96	11·53	−0·97
[ζ Urs. Min.]	1	15	48	45·10	45·54	−0·44	12	+78	11	35·07	35·49	−0·42
α Scorpii	9	16	21	26·32	26·40	−0·08	10	−26	8	26 11	26·42	+0·31
[ζ Herculis]	4	16	36	23·09	23·37	−0·28	8	+31	50	24·96	22·86	+2·10
[κ Ophiuchi]	1	16	51	30·72	30·92	−0·20	5	+ 9	34	45·57	46·34	−0·77
α Herculis	11	17	8	43·18	43·29	−0·11	8	+14	32	26·21	26·76	−0·55
[β Draconis]	2	17	27	29·76	29·79	−0·03	3	+52	23	54·04	54·98	−0·94

Name of Star.	No. of Obs.	R. A. 1870. Oxford.	W.	O—W.	No. of Obs.	Decl. 1870. Oxford.	W.	O—W.
		h m s	°	s		° ′ ″	″	″
α Ophiuchi	6	17 28 54·07	54·05	+0·02	10	+12 39 23 21	25·41	−2·20
[μ Herculis]	7	17 41 22·24	22·26	−0·02	7	+27 47 54·82	56·25	−1·43
γ Draconis	4	17 53 35·06	35·40	−0·34	10	+51 30 18·04	18·40	−0·36
α Lyræ	8	18 32 32·16	32·25	−0·09	15	+38 39 50·39	51·32	−0·93
[β^1 Lyræ]	7	18 45 16·81	16·83	−0·02	13	+33 12 47·74	47·94	−0·20
[δ Aquilæ]	6	19 18 56·53	56·60	−0·07	7	+ 2 51 27·32	28·40	−1·08
γ Aquilæ	2	19 40 4·74	4·79	−0·05	3	+10 17 53·13	54·54	−1·41
α Aquilæ	12	19 44 26·38	26·47	−0·09	15	+ 8 31 36·27	37·35	−1·08
β Aquilæ	8	19 48 55·65	55·70	−0·05	6	+ 6 5 1·49	2·31	−0·82
α^2 Capric.	2	20 10 50·38	50·44	−0·06	1	−12 56 44·68	44·85	+0·17
α Cygni	6	20 36 59·94	60·03	−0·09	14	+44 49 0·07	0·95	−0·88
α Cephei	7	21 15 28·37	28·52	−0·15	30	+62 2 7·22	5·96	+1·26
β Cephei	4	21 26 58·28	58·38	−0·10	27	+69 59 24·46	23·69	+0·77
α Aquarii	6	21 59 6·34	6·41	−0·07	5	− 0 57 1·31	0·86	−0·45
α Pisc. austr.	1	22 50 27·69	27·75	−0·06	1	−30 18 39·11	37·91	−1·20
α Pegasi	7	22 58 17·17	17·23	−0·06	6	+14 30 22·16	24·02	−1·76
[γ Piscium]	9	23 10 25·52	25·56	−0·04	10	+ 2 34 20·61	20·56	+0·05
[ι Piscium]	2	23 32 15·77	15·99	−0·22	2	+ 4 55 16·68	18·55	+1·87
[ω Piscium]	4	23 52 38·14	38·25	−0·11	5	+ 6 8 37·55	37·08	+0·47
α Urs. Min.	71	1 11 16·84	17·49	−0·65	104	+88 36 58·35	59·10	−0·75
δ Urs. Min.	10	18 14 16·13	16·95	−0·82	60	+86 36 20·79	20·33	+0·46

Note by Mr. Drach on the Ancient Rabbinical Cubit-measure.

Mr. Drach remarks on Prof. Wackerbath's Ancient Rabbinical cubit-measure (*Monthly Notices*, Supp. Number, 1873, vol. xxxiii.), that the *Zohar* numbers 6,000 and 12,000, may be partly derived from the Assyrian *Saros*, or the Jewish millennium of 6,000 years, and partly from the Alexandrian Schools. The *Imri Binah* was written by Issachar Baer ben Moses Pethaiah (Prague, 1610). Mr. Drach showed in *Trans. Soc. Biblical Archæology*, vol. i. p. 336, that the ancient Egyptians might have taken the *circumference* of the Earth as an *integral nmmber* of units, and not the inaccessible diameter according to Prof. Piazzi Smyth's Pyramidal notions; thus falling into the same groove as the French metrologues of the eighteeenth century.

1873, *November* 14.

Discovery and Elements of Minor Planet (134)

This planet was discovered on the evening of September 27 by Dr. R. Luther, and was of the 10·11 magnitude. The following observations were made at the Observatory of Bilk on the night of the discovery and on the following evening.

1873	M.T. Bilk. h m s	R.A. (134). h m s	N.P.D. (134). ° ′ ″	
Sept. 27	9 38 37·3	0 8 22·74	82 6 17·9	10 comp.
27	11 24 22·0	0 8 17·26	82 6 22·6	10 „
28	9 27 1·6	0 7 20·36	82 7 14·6	10 „

At the request of Dr. Luther, Professor Argelander and M. Theodor Wolff, of Bonn, have named this planet *Sophrosyne*.

The following elements have been computed by Dr. Tietjen:—

1873, Oct. 13·5 M.T. Berlin.

		°	′	″	
M	=	314	37	42·3	
ω	=	80	45	3·2	Mean Equinox 1873·0.
Ω	=	346	7	5·5	
i	=	11	22	21·8	
ϕ	=	7	22	29·1	
μ	=		862″·7408		
log. a	=		0·409417		

Discovery of Comet VII. 1873.

This Comet was discovered by M. Coggia, at Marseilles, on November 10, and by Dr. Winnecke, at Strasburg, on November 11. M. Coggia remarks that it was, when first seen, of extreme faintness. The following approximate elements have been computed by Prof. E. Weiss from observations made at Strasburg on November 11, and at Vienna on November 12 and 13:—

Perihl.Pass. 1873, December 4·1348 Berlin Mean Time.

		°	′	″	
π	=	94	23	14	Mean Equinox 1873·0
Ω	=	254	14	9	
i	=	27	2	7	
log. q	=	9·83810			

Professor Weiss remarks that the orbit of this comet resembles that of the first comet of 1818, discovered by Pons at Marseilles. Later observations seem to confirm this resemblance.

Mr. Hind has also calculated the following elements from the observations at Marseilles on November 11, and at Hamburg on November 12 and 13:—

Perih. Pass. 1873, November 30·2784 G.M.T.

	°	′	″	
π	83	12	15	Appar. Eq. 1873, Nov. 12.
Ω	249	23	2	
i	30	44	32	

log. q 9·873503

Motion direct.

Mr. Hind also remarks that this orbit has a general resemblance to that recently given by him in the *Monthly Notices* for Pons' comet of February, 1818 (vol. xxxiii. p. 50).

Observation of a Solar Spot. By C. L. Prince, Esq.

It may be worth recording that, on September 18, I observed, on the N.P. quadrant of the Sun, a small black spot of about five seconds in diameter. I have no recollection of having seen a solar spot so nearly circular and almost entirely devoid of penumbra. In a telescope of not more than two inches aperture it might have been taken for a planetary disc.

The Observatory, Crowborough Beacon,
1873, *October* 31.